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Contents

Contents

A.H.M. Jones and the *Later Roman Empire*

By Professor J.H.W.G. LIEBESCHUETZ

The following five papers are published in tribute to the past and continuing contribution to Roman historical and epigraphic studies of Professor John Mann, a graduate of University College and an Honorary Research Fellow of the Institute. The first four were delivered to a one-day conference on 23 May 1992, at the Institute of Archaeology, to mark his seventieth birthday. The meeting was organised by Dr M.M. Roxan, Honorary Research Fellow of the Institute. Dido Clark has edited these papers for subsequent publication. All the contributors are longstanding friends and colleagues of John Mann and the editor is grateful for their ready and prompt collaboration in the preparation. Many persons assisted in making the conference a pleasant and successful occasion. The Institute would like to acknowledge the generosity of the Society for the Promotion of Roman Studies for a most welcome contribution towards the expenses of visiting speakers.

A.H.M. Jones and the *Later Roman Empire*

by Professor J.H.W.G. LIEBESCHUETZ*

In 1984 Alexander Demandt published *Der Fall Roms*. Over six hundred pages, he gives a systematic survey of a huge range of explanation for the fall of the Roman Empire suggested by scholars from the Renaissance to the present. For a professional historian this is deeply depressing reading. It is a demonstration of the ephemeral nature of historical research and of the impossibility of reaching definitive answers to the great problems of history.

But the reason why the book is so depressing is not because it shows that historical research is futile but because it provides condensed summaries of conclusions of historical research. The result of scientific research can be summed up in compact formulae. Historical research cannot. If, instead of looking at a compilation of summaries, you look at the individual studies that is not depressing at all. You become aware of what highly intelligent people of a particular time have thought about these momentous events, which factors they considered important, which they neglected, and how their selection was influenced by concerns outside their scholarship. You become acquainted with the thoughts of the scholars themselves, and also with the concerns of the world which had produced the scholars and which they were addressing. It is from this point of view that I intend to approach the work of A.H.M. Jones.

Of books on Ancient History that have appeared in my lifetime and in a language I have been able to read, Jones' *Later Roman Empire* is to my mind much the greatest. This is not only, I think, because A.H.M. Jones was my teacher, as he was John Mann's and of many of us here; the *Later Roman Empire* dominates the literature on the Later Empire during our times much as Mommsen's *Staatsrecht* did literature on earlier periods of Roman history in Mommsen's time and since.

Greatness apart, the *Later Roman Empire* has another feature in common with the *Staatsrecht*. It has assembled an incredibly vast amount of material in a systematic, logical and easily consultable way. Jones did for the Later Empire what Mommsen had done for the Republic. It is a safe prophecy that both books will be consulted as long as Ancient History is studied at universities. The *Later Roman Empire* is indeed a *monumentum aere perennius*.

A great deal has been written about Mommsen but very little about the background of the writing of A.H.M. Jones. When preparing this article I found articles by Peter Brunt (1981), J. Crook (1971), A. Momigliano (1965) and P. Brown (1967) helpful. The first three knew him as colleagues. None has anything about his intellectual development. Jones was, as many here must have experienced, a very private person indeed. I made some enquiries about papers that he might have left, but without result. But the question why or how Jones came to

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1

write as he did is an interesting one; Jones' writings stand apart from the books on Ancient History produced in Britain by his contemporaries.

In the thirties Jones began to write large books at a time when British scholars on the whole wrote text books and articles on a small scale (see reviews in *JRS* and *JHS* of the 1920s). He was concerned with the Hellenistic and late periods when most work was on the classical period. His interest was in the provinces, when others mainly wrote on Rome. He was concerned with administrative, social and economic history when the main emphasis was on political history, and in the case of Hugh Last, constitutional history. How did Jones become Jones?

There was a British tradition of late Roman and Byzantine history represented by Bury and Baynes. These two undoubtedly great historians were quite different from each other, and also totally different from Jones. According to John Crook (letter to author) Jones used to say that he intended the *Later Roman Empire* to be a continuation of Rostovtzeff's *Social and Economic History of the Roman Empire*. This may seem surprising, because the two books are different in so many ways. Nevertheless, it is likely that Jones' research interests were decisively affected by the appearance of Rostovtzeff's book in 1926, the year in which Jones took Greats and started on research. Momigliano has recorded the unforgettable impression that Rostovtzeff's book made on its first appearance with its plates of archaeological evidence, its overwhelming learning and uncanny gift of bringing things ancient to life (1954: 334).

There is internal evidence for dependence. The *Later Roman Empire* starts more or less where the *Social and Economic History of the Roman Empire* finishes. Like Rostovtzeff, but quite in his own way, Jones was interested in periods later than the classical, in the provinces and their cities rather than the capital, and in social and economic history rather than politics and wars. Besides, Jones and Rostovtzeff had a basic idea or insight in common: both postulated a sharp conflict of interest between peasantry and the inhabitants of cities, particularly the ruling classes of cities, who built their elegant way of life on rents drawn from the peasantry. Rostovtzeff saw the crisis of the third century as a consequence of resentment by the peasants of what he calls bourgeois civilisation. Jones saw the same conflict from the peasants' point of view. In his reconstruction, exploitation of the peasantry led not to violent reaction but to apathy and demographic decline (1964: 810–12, 822–3).

Jones may have been impressed and inspired, but his way of writing history was from the beginning very different from that of Rostovtzeff. Rostovtzeff was learned, vivid and stimulating, but in his text it is almost impossible to discover where source-based description ends, and imaginative reconstruction begins. So for all its merits the *Social and Economic History of the Roman Empire* has not and is not likely to become a timeless reference book. As he repeatedly insisted to his students, Jones made it a matter of high, even moral principle, to set out the evidence fully and clearly, so that the reader could assess the argument at every stage.

The difference was no doubt in part a matter of personality, but certainly also of national academic tradition. Jones wrote in the educational tradition defined in an interesting lecture of G.H. Stevenson, 'Some Reflections on the teaching of Roman history': 'the student of Roman history, if properly taught, is kept constantly in touch with the evidence, on which consecutive historians have based their assertions. He is...to accept nothing at second hand.

He learns to cultivate the critical spirit and to weigh as well as measure his evidence, making allowance for the prejudices of contemporaries' (1922: 195). Rostovtzeff had not been subjected to the disciplines of Greats, and not surprisingly his work was received rather coolly at Oxford. Hugh Last (1926: 121) noted that 'Rostovtzeff's views are by no means always easy to express. The causes of the difficulty are two. One is the readiness to use terms which are undefined... The reader is left wondering what precisely capitalism connotes and what qualities give soundness or unsoundness to a capitalistic system. The second is that Rostovtzeff seems sometimes to be in two minds if he does not actually fall into open contradiction.' Jones' method is in a sense a continuous correction of these failings of Rostovtzeff.

In a central area of crucial social history, Jones came to a conclusion which was diametrically opposite to that of Rostovtzeff, and that was in his assessment of the fundamental nature of the ancient city. It is universally agreed that ancient civilisation was a city civilisation, and that its expansion was closely linked with the development of the city, and the expansion of a network of cities. But according to Rostovtzeff city life depended on what he calls its bourgeoisie, that is a middle class whose wealth like that of the modern middle class was derived to a great extent from trade and manufacture. 'As far as I can judge from the evidence, the main source of large fortunes was commerce' (1926: 145, cf. 176). Jones' works on the cities of the Roman East had led him to the conclusion that the leading citizens had very little to do with trade or manufacture, and that individuals whose life was concerned with trade and/or manufacture played a very unimportant role in both civic and imperial politics (1940: 164–5). Later Jones showed that the revenue from imperial taxation of trade was insignificant compared with that derived from taxing agriculture (1955). Jones put forward his theory of the unimportance of trade in the ancient world in his inaugural lecture at University College London (1948). As Jones saw it the ancient city was essentially an administrative centre, where the élite of the region lived, governed and maintained itself in style at the expense of the peasants. Thus the wealth of cities came largely from agriculture. As economic centres cities were of minor importance. The city was essentially a consumer city inhabited by 'idle mouths' to quote a favourite phrase (1964: 1045). This view is now widely held.

It helped acceptance of Jones' view that a few years later a very eloquent and articulate scholar called Moses Finley began to propound a very similar view of the consumer city. Reading Finley one might deduce that he had simply taken over Jones' view. In fact the editors' preface to Finley's *Economy and Society in Ancient Greece* shows that Finley, starting from Max Weber, had reached the same result by a totally different route (Weber, 1924; Finley, 1973, 1981: ix–xxvi). Compared with Rostovtzeff, the Jones–Finley view undoubtedly meant progress. As against their great predecessor. Jones and Finley were clearly right. But today the theory of the consumer city represents a dead end. The ancient city too had an economic basis, but it is not one which can be reached by Jones' methodology, or through the evidence Jones chose to investigate.

The differences between Jones and Rostovtzeff over the nature of the ancient city and its élite are not simply a result of different methodologies and academic traditions. They reflect the very different life experiences of a Briton and of a Russian in the mid 1920s. Rostovtzeff wrote as a refugee from Russia, after imperial Russia had lost the war, and the bourgeoisie

had been smashed by the Communist Revolution. His great *History* is an elegy for the bourgeoisie of imperial Russia. His famous concluding words are deeply pessimistic, not to say apocalyptic: 'Violent attempts at levelling have never helped to uplift the masses. They have destroyed the upper classes, and resulted in accelerating the power of barbarization. Is not every civilization bound to decay as soon as it begins to penetrate the masses?' (1926: 487).

In Germany defeat inspired a similar end of civilization view, and everybody aspiring to be an intellectual bought two volumes of Oswald Spengler's significantly entitled *Der Untergang des Abendlandes, The Decline of the West* (1919–22, 1926-9), a highly intelligent, pseudo-historical, semi-prophetic book. He actually started work on the book before the First World War (1966: 26), but it achieved its enormous readership in the years following the defeat of 1918. The impact was not limited to Germany. It reached England, where the book impressed Arnold Toynbee, and helped him to sort out his ideas for the *Study of History* (McNeil, 1989: 98–102), a vast historical enterprise in which the basic unit of study was not the nation state but a whole civilization, and in which the stages of development of different civilizations were treated as in some sense parallel and contemporary (Toynbee, 1948: 9–10). But thanks to its origin in England, Toynbee's study of human civilizations is a liberal and optimistic work. Doom and decay are avoidable. Survival and creativity can be ensured by meeting each challenge as it comes with enough flexibility to find the appropriate response. Toynbee was a progressive, a liberal and an optimist with regard to the prospects of democratic politics, and the possibility of using state action to improve the quality of life for the great majority of the population. Hugh Gaitskell, who overlapped with A.H.M. Jones at New College, has described the liberal atmosphere among undergraduates. 'In the heavenly freedom of Oxford [revolt] took the form of an outburst of scepticism, a mistrust of dogma, a dislike of sentimentality and of over-emotion and prejudice or violent crusades. We were therefore suspicious of general ideas...' (1967: 6–7). I think that all these attitudes can be found in Jones' writings. At the same time it remains likely that Jones' decision to take his research into late Antiquity owes something to the interest in the decline of civilization which was so strong in the twenties.

About Jones' politics, his daughter, Mrs Cordelia Gidney writes as follows:

> As an undergraduate he was chairman of the Labour Club, but I do not think he played an active role in the party at any later date. It was always a point of difference between my parents, and for the sake of peace in the home he refused to tell us children how he voted, reminding us that the ballot was secret. But he was always ready to give us an impartial explanation of economic or political policies. I only know that both my parents voted Labour in 1945, but thereafter my mother drifted back to the right and was always grumbling about his left wing friends.

With regards to politics, Jones evidently was the same at home to his children as he was in class or seminar to his students. He certainly taught history in a way that would have been approved by Max Weber (1973: 600–1). He did not take political sides, but was very sensitive to the implications and consequences of different constitutional arrangements, and was careful to point them out. So he was the first to my knowledge to try to show how Athenian

democracy, with its extreme degree of participation by the *demos* in its own government, was actually made to work (1957: 99-133). He was also the first to draw attention to the process by which the Romans killed popular politics in the cities of their empire (1940: 120–1,170–1) with the result that first civic patriotism, but eventually also imperial patriotism, died from inanition (1940: 45–6, 301–4). A.L. Rowse remembers that Jones' dislike for Julius Caesar seemed almost a personal dislike.

In the 1930s, the years that Jones was achieving academic maturity, the intellectual foundation of what was to become the welfare state were being laid, and when he completed the *Later Roman Empire* the welfare state was in existence. As far as I know, Jones had no part in these discussions, but it is probably no coincidence that one of his central interests was administration, what it could do, what its dangers were, and above all how it worked. These are all questions of central importance for the creation of a welfare system based on government action and planning. Rostovtzeff had completely lacked this interest: 'being supreme and omnipotent, and not subject to any control the bureaucracy gradually became utterly corrupt and dishonest and at the same time comparatively inefficient in spite of the high professional training of its members' (1926: 459–60). Rostovtzeff disliked bureaucracy, presumably he associated it with the officialdom of Russia whether imperial or communist. Jones' totally different attitude is brought out by his assessment of late Roman officialdom:

> It is easy to poke fun at the byzantine bureaucracy, but cumbersome and corrupt as it was, it served some useful purposes. The emperor relied on the clerical grades, perhaps not altogether in vain, as a check against the far more arbitrary extortions of the provincial governor. The East survived partly because it was able to mobilise a larger proportion of the available wealth and manpower—because in the East the powers of the state were… wielded by bureaucrats, men like Marinus the Syrian and John the Cappadocian who lined their own pockets…but also filled the treasury. (1949: 55)

To sum up, I am suggesting that Jones' interest in the working administration, and the nature of the topics he selected for investigation suggest that he was concerned with the same kind of problems as those that faced reformist Labour politicians. That is surely the reason why in his very perceptive article in *Oxford Magazine* Momigliano (1965: 264) thought that the *Later Roman Empire* was 'in the direct line of the Webbs and the Hammonds, indeed of Booth and Beveridge'.

It still remains a question what factors and what experiences shaped Jones' outlook in detail. His father was a journalist said to have represented 'the ardent radicalism of his day in Wales' *(Times* obituary 16.6.39). He was for many years, 1908–24, successful editor of the liberal *Statesman* of Calcutta. Meanwhile Jones was being educated with the help of scholarships at Cheltenham and at New College. From 1926–9 he was a fellow of All Souls and this gave him a great deal of freedom to explore in multiple directions and even to take on other works, for instance teaching and digging. According to A.L. Rowse, he had been interested in archaeology from school days, and particularly in the Middle Ages and the college buildings whilst at Oxford. In 1927 he spent six months excavating at Constantinople, and some more time at Jerash in the summers of 1928 and 1929. 1t was probably also in 1928 that he dug at

the monastery of Euthymius in Palestine together with Derwas Chitty, author of *The Desert a City*. Chitty was a close friend from undergraduate days until Jones' death. Mrs Gidney writes: 'Another thing I guess he owed to Derwas Chitty was the fact that he took the church seriously; although styling himself an atheist he never assumed that believers were necessarily either knaves or fools. As an illustration of what I mean, my father greatly admired the Claudius novels of Robert Graves, but not the Belisarius ones, saying that Graves had entirely failed to understand the mind of the later period.' From 1929 to 1934 Jones taught Ancient History at the University of Cairo. He used the opportunity to visit Near Eastern sites by car. Here is Mrs Gidney's account of this part of her father's life:

> He had married, and planned to start a family and needed to supplement his slender income of £300 per annum from All Souls; spells of work on excavations allowed him to do this. Certainly he did not choose the post in Cairo because he wanted to work there—he had applied for various posts in England without success and viewed Cairo as an exile. I quote from a letter to my mother written from Jerash, May 28, 1929:

> 'The fact is that yesterday I received a telegram from you advising me not to accept the Egyptian offer and today I received a telegram, reply paid, from the Egyptian University, saying "will you take £540 a year? Yes or no." I was very glad to be sure of your support in making my decision; and so I replied, I trust in the spirit of the Egyptians, if not according to the letter of the demand, "I will accept £720" which is the maximum scale for a lecturer you may remember. I fear that our prospects in Egypt, however are closed. Crowfoot—who was in charge of the Jerash dig—asked me why I did not ask Richmond for a job, he says he has several vacant and that the salary would be better than £540 p.a. What do you think of the Palestine Archaeological service? I think it would be quite good.'

> In the event Cairo accepted my father at his own valuation and my mother was to look back on the five years spent there as a golden age before her troubles began. I think my father in retrospect saw the value of teaching as against the archaeology service. I know he said in advice to others that teaching some vast swathe of history to undergraduates would make them better historians than concentrating on some small PhD subject. After five years of it, however, he needed time to get down to writing the books on the cities so he and my mother, with their first baby, returned to Oxford to live on £300 from All Souls.

During the war he worked in the Ministry of Labour, and was concerned with manpower planning. To this experience he might have adapted Gibbons' comment that 'the captain of the Hampshire Grenadiers—the reader may smile—has not been useless to the historian of the Empire' (1984: 128). He noticed a similarity between the wartime regulations and restrictions imposed for the direction of labour and the late Roman Laws tying workers in certain occupations to their trades. It is the case I think that in the *Later Roman Empire* Jones took a more positive view of the late Roman bureaucracy than he had done in the Greek city. In *The Greek City* Jones concluded that interference by the imperial administration killed local initiative and patriotism, with the result that there remained no force to keep the Empire alive (1940: 303-4). In the *Later Roman Empire* he placed principal responsibility for the fall

of the Western Empire on barbarian invaders, and argued that the survival of the East was to a significant extent due to the efforts of its bureaucracy (1964: 206–7, 606). Nevertheless, too much should not be made of this. To discover why the Empire fell had never been one of Jones' principal objectives. He was much more interested to explain how it was kept going for so long. In any case the impact of his war-time experience in the Ministry of Labour can only have been marginal. By the time of the war Jones was already a mature historian and had written the *Cities of Eastern Provinces* (1937) and *The Greek City* (1940).

At the start of the lecture I promised an investigation into how Jones became Jones. I am afraid I still owe you a definitive answer. I cannot tell you who, if anybody, had an impact on the intellectual formation of Jones, comparable to that which Jones had on so many of his pupils, including not a few in this room.

Abstract

Historical research is ephemeral and reflects the thoughts and interests of scholars and the concerns of the world of their times. Nevertheless, Liebeschuetz considers that A.H.M Jones' Later Roman Empire is the greatest book on ancient history to appear in his lifetime. What made Jones different from his contemporaries and how did he attain his point of view? His work was intended to be a continuation of Rostovtzeff's but his attitude and methods were different. He set out the evidence in full so that readers could judge for themselves. His greatest difference from Rostovtzeff was in the importance placed upon agriculture, rather than trade and manufacture, as the basis of wealth of the ancient city. Reticent about his political views even to his family, Jones was certainly left of centre but, even aided by anecdotes about his life by his daughter, his reserve leaves many questions unanswered about the influences that shaped his intellectual approach.

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Coin inscriptions and language

by JOHN KENT*

During the night of 26–7 June 363 the Emperor Julian died of wounds inflicted by an unknown hand, and a hastily assembled junta of the senior officers of his army and administration decided on the proclamation of Jovian, the *Primicerius Domesticorum*, as his successor. The army was spread out in a column four miles in length, and when in the morning the new emperor was paraded along the line for acclamation, Ammianus (25.5.6) records that as the more distant units heard the shouts, they were unable to tell whether Julian still lived or whether they had a new ruler until Jovian actually came into view, 'gentilitate enim perciti nominis, quod una littera discernebat'; the sounds of **o** and **u** were indistinguishable. If this should seem odd, let us remember that we all speak of *London*, (*Lunden* on coins inscribed in English down to 1279, thereafter *London(iae)*, since legends were now in Latin), *money*, *honey*, *monks* and *coming* etc., and many of us still of *covert*, *frontiers*, *constables*, *Coventry* and *Covent Garden*. The Portuguese, too, in some contexts pronounce **o** as **u**, or at least as **oo**. Nor are these the only vowels we confuse, as the names of *Derby* and *Hertford* remind us. Do not think that this particular ambivalence is recent; coins of Edward the Confessor (when they do not adopt an archaic OE form) may spell his name EDWERD or EDWARD (Figs 1, 2). The complex interrelationships of grammar, orthography and pronunciation have long been a matter of special interest to John Mann (1971: 218–24) and it is therefore a pleasure to offer these observations to my fellow student and old friend. To the best of my knowledge, linguists have never exploited, nor even explored the well-dated material offered by coin-legends. This is not the stuff of conventional numismatics, whose classically trained exponents have usually regarded non-classical forms as either barbarisms or engravers' errors— 'mere illiteracies'—rightly, perhaps, in a technical sense.

The language of coinage, though not necessarily that of tokens, is for the most part the product of a centralised officialdom, and like all officialese, is conservative in form and content. The generality of coin inscriptions is therefore of little use to us. We must seek out those which are exceptional, and particularly those which emanate from a more limited and perhaps less formally literate authority. Carausius is a good instance, and his coinage in fact furnishes many non-classical readings. Forms which are manifestly the result of an engraver's error, such as transposed or unsystematically omitted letters, must be discarded and even those that might be significant, but which could possibly be accidental in origin. There remain a great number of coin legends which bear on the problems of relating the living speech to the rarified languages of literature, law and religion. 'What is correctness of speech, anyway',

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asked St Augustine (*de doctrina Christiana* 2.13.19), 'but the observance of the usage of others, confirmed by the authority of the speakers of old?' 'It is better', he opined (*Serm.* 3. 6), 'that you should understand my barbarisms, than that my learned speech should make me unintelligible to you.' 'Nec quaerant grammatici quam sit latinum', said he (*Serm.* 299.6) of a word that he and his critics had (wrongly) taken to be a Christian neologism. There must have been a world of difference between actual delivery and his polished publications.

The non-classical forms at which we shall look are widely spread both in space and time. They illustrate the ways in which those who drafted and engraved coin legends perceived the sound-values of the letters they inscribed. With the aid of modern languages, they help us to form a view of what sounds may have been intended. Far less linguistic change appears to have taken place over two millennia than conventional wisdom would have us believe, and the literature of the classical world is a poor guide to the realities of the spoken tongue. This is of course a commonplace, but its implications do not seem to have been fully absorbed. Little of what is noted here cannot already be found in Ernst Diehl's useful compendium (1930). There are, for instance, examples of the interchangeability of οι and υ on Greek-language coins both of the third century and of the tenth and eleventh centuries, and it should be asked how this relates—as it surely must—to the corresponding interchange of oi and u on Latin inscriptions of the Roman republic, as well as to those on Anglo-Saxon coins of the tenth century. The intended pronunciation must have been the same at any one of these moments in time and place, and the different orthographies merely a reflection of long-standing rival teaching practices. The same phenomena must underlie the various spellings of *Invicta Roma* on coins struck in Ostrogothic Italy over a relatively short period in the late fifth and early sixth centuries. As well as the classic INVICTA, we find INBICTA (implying a pronounced V), IMVICTA (which implies a pronounced B) and INVITA (for *Invitta* as in modern Italian, presumably what was actually spoken) (Figs 3–6). Modern Spanish suggests that the pro-nounced distinction, where it existed, between **B** and **V** may have depended on position; e.g., *Victoria* = [bikto:rya], but *la Victoria* = [vikto:rya]. **B** and **V** may thus be orthographically interchangeable, both (in Spain, but not Italy) having the value [b] when standing initially in words not preceded by a vowel or nasal. In Visigothic Spain it was not uncommon to render Gothic **W–** by **UB** in place of the more usual **UU** (Dietrich: 1862) giving rise to some grotesque forms.

Before offering a more general summary of the evidence, it is proposed by way of example to consider in detail what the coins made of the names of two empresses, Herennia Etruscilla, wife of Decius (249–51), and Julia Soaemias, mother of Elagabalus (218–22). Etruscilla had a perfectly normal Roman name, but outside Rome itself it could present difficulties. The regular transliteration Ἐτρουσκίλλα is indeed found often enough in the eastern provinces; however, there is uncertainty over the internal vowels, with ω, ο or υ for ου, and υ for ι. The initial vowel is often hypercorrected to Ἀι—, or treated as prosthetic and omitted altogether, **TRVSCILLA** or Τρουσκίλλα (Figs 7, 8). Hesitancy over the length of a Roman vowel is also found in the occasional incidence of ω for ο in the name of Domitian, and the converse in that of Antoninus. Soaemias had an exotic name, and the Greek east had problems as soon as anything more than Σοαιμιὰς, the straight transliteration of the official Roman spelling, was

attempted. In Syria, where the name should have been best known, they tended to write **SEMIA** or **Σεμιὰς**, and this may well represent 'correct' pronunciation, for she appears in Eutropius (VIII.13) as **Semiasyra**—'Semia the Syrian lady'. Hearne put it well in the footnotes to his 1703 edition: *Ingens in hoc nomine variatio.* Elsewhere the **o** might be replaced by **ου** or **υ**, the **αι** by **ε** or **ι**, while the **ιας** might be written **ις** or (less commonly) **ια**. The diphthong in her name appears therefore to represent a short vowel i.e. is in effect a hypercorrection; as we shall see below, the terminal **S** was probably scarcely sounded. The varied forms—Dio (LXXIX.38.4) preferred **Σοαιμὶς**—illustrate the tensions between attempts to reproduce official orthography, hesitancy over the sound-values of the written vowels, and desire to compose a form which would be correctly pronounced.

The following sample of the evidence available from the coinage is of course no more than representative. It is arranged phonetically rather than chronologically, and with a minimum of comment; it mostly speaks for itself, and no attempt has been made to reproduce actual letter forms, apart from the curved **Ч**.

Diphthongs

E for AE: CESAR Philip II, DIANE, EMILIANO Aemilian, SECVLI Probus, Constantine I, CELEST Urbica (Carinus), PVBLICE Constantine I, Johannes, Valentinian III, Anthemius etc., ELIA Licinia Eudoxia (Valentinian III), ΚΕΣΑΡ = Caesar: Vespasian onwards, ΕΛΙ = Aeli (us): Antoninus Pius, ΚΕΛ = Cael(ius) Balbinus

and its hypercorrection.

AE for E: AETRVSCVS Herennius (Decius), GALLIENAE AVGVSTAE Gallienus (see *Numismatic Chronicle* 1973, 64–8), AEQVIT = *Equit(um)* Postumus, CONSAECRATIO Claudius II, AERCVLI, PIAETAS Probus (Fig. 9), Carausius, NVMAERIANVS Carus, RAEDVX Carausius, SAECVRIT, BAEATISSIMO Diocletian, AELENA Helena (Constantine I) (Fig. 10), ΑΙΤΡΟΥΣΚΙΛΛΑ Etruscilla (Decius) (Fig. 7).

A for AV: AGV Lucilla (Verus), AG (ustus etc.) Constantine I, Theodosius II, Valentinian III, Leo I, Libius Severus etc.; a form **AVSTVS** (Romulus) shows a complete reduction of the name (as opposed to the title) to something approaching the pronunciation of the Italian city of *Aosta,* the initial **A** and **V** representing different syllables. See below under **G**. ΑΓΟΣΤ, ΑΓΟΥ Domna (Severus).

O for AV: ΩΛΟΥ = ΑΥΛΟΥ Vitellius.

V for OI: this and its converse are common in Republican inscriptions; the numismatic evidence is mainly Greek. OMONUIA = OMONOIA third century, DЧLЧ = δοῦλοι, PISTЧ Theophilus (Fig. 11), PISTV Basil I (Fig. 12); PISTOI and PISTV may be found on successive issues of Basil II. DESPVNA Theodora

(Michael III) (Fig. 13). Note that the Thracian name, Greek ΡΟΙΜΕΤΑΔΚΗΣ is rendered by Latin *Rumitalca* (Amm. 26.8.1 or *Rometalca* (Dessau *ILS*.701). Note also the Anglo-Saxon moneyer BOGA/BVGA/BOIGA/BOIA etc. These differences must be orthographic rather than linguistic.

Vowels

I for E: PRINCIPS Domitian, CVPRIS(senia) Etruscilla (Decius), TRIB(onianus) Gallus, SINATVS, CESARIA = Caesarea, third century, LITITIA = *Letitia* = *Laetitia* Carausius, COMIS, EQUIS, FIDIS Constantine I, SEVIRVS Libius Severus, EVFIMIA Euphemia (Anthemius), ZINO Zeno, RIX Ostrogothic, ΟΥΙΡΩ Verus.

E for I: QVENTVM Hostilian (Decius), HELARITAS Postumus, SOLE Maximin II, FELEX Honorius, REGES Suevian, VECTORIA Libius Severus, ΒΡΕΤΑΝΝΙΚΟΣ Britannicus (Claudius), ΔΟΜΕΤΙΑΝΟΣ Domitian.

E omitted: ΤΡΟΥΣΚΙΛΛΑ Etruscilla (Decius) (Fig. 8).

I for V: EXERCITI   Valerian onwards. This occurs much earlier, in Republican inscriptions and Petronius, and was probably facilitated by the fall of the terminal S in pronunciation; see below.

V for I: EVFVMIA Euphemia (who is sometimes EVFIMIA), see above.

I omitted: ΓΑΛΛΗΝΟΣ Gallienus (Fig. 14). Note the hypercorrection LVCILLIA (temp. Verus), suggesting the pronunciation of LL as in modern Spanish, VRTVS Valentinian, III, DOMNO Odovacar, Theia, LEONS Leo I, ZENONS Zeno.

I intruded: VRBIS   Valentinian III onwards.

O for V: POP = Pub(lius) Geta, FOL ION = Ful(vius) Iun(ius) Macrianus, SECORITAS Jovian, ROMANORO Zeno, AVGVSTOROM Ostrogothic, ΑΥΓΟΣΤΑ Domna (Severus), ΠΟΠΛΙ = Publi(us) Geta, ΔΙΑΔΟΜΕΝΙΑΝΟΣ Diadumenian (Macrinus).

V for O: SVAEMIAS Soaemias (Elagabalus), CVRNE(lia) Salonina (Gallienus), CVELA Coela, third century, RVMANO(rum) Carausius, VITVRIA Nepos, FLVREAS Totila (Fig. 15), ΜΥΛΤΥΣ Philippicus (Fig. 16). A rounded U for the vowel is found occasionally from the time of Elagabalus, and becomes more frequent from the mid fifth century.

O omitted: a Greek phenomenon. ΑΥΡΗΛΙΣ Marcus (Fig. 17), ΛΟΥΚΙΣ Verus, ΔΕΚΙΣ Decius, STAVRACIS Stauracius (Nicephorus I) (Fig. 18).
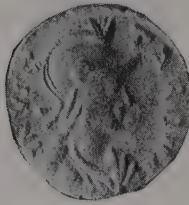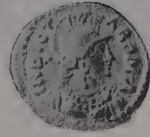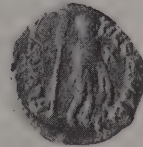
Figs. 1–16   Coin inscriptions.

V omitted: EXERCITVM Honorius. This may be an orthographic confusion due to unsounded terminal **M** and **S**. ΤΡΑΝΚΛΙΝΑ = Tranquillina (Gordian III); compare TRANQLITAS, the London mint spelling of TRANQVILLITAS (Constantine I).

Consonants

B written R: this is simply a letter form. LI**R** = *Liberalitas* Severus, LI**R**IVS Severus, CONO**R** = CONOB Leo II, Zeno, PV**R**LICAE Leo I etc.; the form is common on Byzantine coins down to the eleventh century. There is a hypercorrect VICTO**B**IA Basiliscus etc.

B for V: IO**B**I Severus, IV**B**ENTVS Gallienus, **B**ENERI Salonina (Gallienus), RENO**B**ATIO Magnentius, LI**B**IVS Severus, IN**B**ICTA Ostrogothic (Fig. 4), ΛΙ**B**ΙΑ Livia (Augustus), ΟΚΤΑ**B**ΙΑ Octavia (Nero), **B**ΗΡΟΣ Verus, ΣΕ**B**ΗΡΟΣ Severus, **B**ΕΙΒ = Vib(ius) Gallus, **B**ΟΛΟΥΣΙΑΝΟΣ etc. Volusian, **B**ΑΛΕΡΙΑΝΟΣ Valerian.

V for B: VIVIO = Vibio Gallus, OR**V**IS Theodosius II (Fig. 19), Marcian, NO**V** CAES Constantine II, Zeno. For confusion of **B**, **L**, and **V** see below.

C for QV: A**C**VLIA = Aquillia (Elagabalus), TRAN**C**VLLINA = Tranquillina, normally ΤΡΑΝΚΥΛΛ[Ε]ΙΝΑ in Greek (Gordian III), E**C**VITAS Carausius.

QV for C: **QV**IRIACVS Merovingian.

CI for TI: PASSEN**CI**O = Pascentio, AE**CI**VS = Aetius Merovingian. Note that MARCIANVS is normal; Dessau *ILS*. 824 suggests that the name is to be pronounced as a trisyllable.

CT reduced to T: INVI**T**A = *Invicta* Domitius Alexander, Ostrogothic (Fig. 6), AV**T**OR Contorniate medallion, SAN**T**I Merovingian.

DI for GI: RE**DI**NA = *Regina* Dryantilla (Regalian), ELI**DI**VS, REMI**DI** Merovingian. **DI**VSTINIANϒS for Justinian II poses a problem. His successors used initial **D** for *Dominus,* but under Justinian it followed a period when his name was without preceding title; it might indicate pronunciation; cf. the doublet IOVIS/**DI**OVIS, and the contrasting 'George' and 'Geordie'. A Merovingian coin writes SCI IOR**GI**I.

GN for DN: ARIA**GN**E (but not on coins); for the problems surrounding the orthography of the name of Zeno's wife, see Kent (1991, 35–44). A form resembling **G** apparently a reversed **δ**, occurs in the eighth century (Leo III etc.), but is probably unrelated.
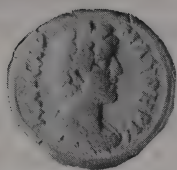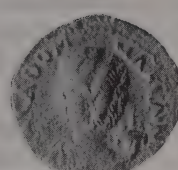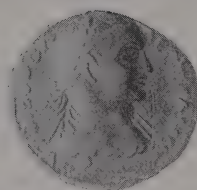
Figs. 17–26   Coin inscriptions.

G reduced to intervocalic **Y**, and consequent hypercorrection: IVLGA Domna (Severus) (Fig. 20), ORIGENTIS = *Orientis* Aurelian, Romulus **AVSTVS** = Augustus, common on Merovingian coins: AMIANIS = AMBIGANES = Ambianis (Amiens), **A**GITIUS = Aetius emphasises that **AE** is a disyllable, not a diphthong. Cf. also OE names in ÆÐEL–, written ÆG[E]L– → ME AYL–; the early orthography was long maintained by archaising royal names like ÆÐELRÆD, but after his death, moneyers' names such as ÆÐELWINE were rapidly reduced to ÆGELWINE (Figs 21, 22). Royal names seem often to have retained early

spellings cf. DAGOBERT, who had an official DAOBERT, and EADWEARD the Confessor, who may also have the less 'literary' forms EDWERD, EDWARD (Figs 1, 2).

H omitted: in the grammarians' view *H littera non est,* and it was regularly omitted on provincial issues of Hadrian and Herennia Etruscilla (Figs 7, 8); it might be accompanied by hypercorrection e.g., AERCVLI Probus, AELENA Helena (Constantine I) (Fig. 10); ERCVLI Aemilian, ONORIVS Honorius, ORATIVS Contorniate medallion; the confused ANTEHEMIVS (Anthemius) is no doubt an attempt at correction; its hypercorrection is rare: HORIENS Gallienus, HETERNA Contorniate medallion.

H sounded as guttural, for Greek **X**: MIHAEL Michael III (Fig. 23), ADELHIS, ERMENRIH Beneventum; also found on the thirteenth-century spelling of the mint of Lichfield: LIHEF.

L confused with **R**: CELERI = Cereri Niger, ΦΟΥΡΒΙΑ Fulvia Plautilla (Severus), ΦΟΥΛΒΙΑ Furia Tranquillina (Gordian III); LERIGIO = *Religio* Valerian, is a simple metathesis.

LL see **I**

M omitted at end of word: ROMANO for *Romanom* (Fig. 24) Republic, early third century BC, CERERE Domna (Severus), ROMANORV Constantine I, Ostrogoths, ROMANORO Zeno.

N omitted: Quintilian said that *consul* was spoken *exempta littera n* and Varro said *mesa* rather than *mensa*: RESVRGES = *Resurgens* Galba, ABVNDAT = *Abundant(ia)* Diocletian, ΟΥΑΛΗΣ = Valens Hostilian (Decius), ΚΩΣΤΑΝΤΙΟΣ Chlorus. An early loss of *n* explains why the Greeks called the *Romani* Ῥωμαῖοι; that was what they heard.

N for **GN**: PROPVNATORI Gallienus.

S omitted at end of word: VIRTVS CAESARI N Crispus (Constantine I), CONSTANTINOPOLI at a group of mints, for Constantinopolis, the usual spelling; see Kent (1987, vii) (Fig. 25). Coupled with the confusion of **E** and **AE**, the hypercorrection of this error readily explains forms like AVGVSTES Helena (Constantine I), also found on inscriptions cf. Diehl (1913: 71).

S rendered by **X**: MVNAXTERIVM Merovingian, probably explains TAXCIAVANVS = Tasciovanus.

X rendered by **S**: PAS: Anthemius; *Pax* gave mediaeval engravers great trouble, in their attempts to ensure 'correct' pronunciation e.g., PACX, PAXS; cf. the London inscription *RIB* 9: OLVSSA = Ulixes.

SS for **SC**: PASSENCIO = Pascentius Merovingian.

T and **C** confused: see above; also ΟΥΛΕΣΤΙΑΝΟΣ = Volusianus (Fig. 26).

Abstract

Coin legends offer an interesting sidelight on the pronunciation of Latin. The language of coinage is mostly produced by a central authority but forms which are exceptional, emanating from a more limited and perhaps less literate authority, are illuminating. Engravers' errors and accidents excluded, the non-classical legends demonstrate the ways in which those who drafted and engraved them perceived the sound values of the letters they described. A language like Latin that has lost terminal m and s, and does not distinguish between e and i, and o and u, cannot have still been significantly inflected in speech, whatever may have been retained in formal, especially liturgical use, by both pagans and Christians, and in literature. Twenty-six coins are used to illustrate vowel and consonant substitutions, which suggest that perhaps Romans may have really spoken Italian, however they tried to write it.

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Cavalry on frontiers: Hadrian to Honorius

by DAVID J. BREEZE*

The north British frontier is taken as the starting point for this discussion of cavalry on frontiers. It is noteworthy that less cavalry was stationed on the Antonine Wall than on Hadrian's Wall (Maps 1 and 2; cf. Breeze and Dobson, 1987: 54, 107). The governor appears to have had the same troops at his disposal but the same regiments clearly were not moved forward from a fort on one Wall to the appropriate fort on the other, as happened on the German frontier where a move from one line to another also occurred under Antoninus Pius (Filtzinger et al. 1986: 79). Other considerations presumably played a part in the different troop dispositions on the two Walls and there are a range of possible factors from which to choose. My earliest thought was that the lack of cavalry on the Antonine Wall indicated that the main purpose of the troops on this Wall was local patrolling; later, however, an alternative possibility seemed more probable—that the answer lay in the difference in the terrain through which the two Walls ran (Breeze and Dobson, 1969–70: 116, 1976: 95–6). The line chosen for the Antonine Wall was good from the Roman point of view. For 30 of the 37 miles of the Wall the line ran along the southern lip of the broad, boggy valleys of the rivers Carron and Kelvin (Map 1). These valleys were not good cavalry country. The location of the only three units containing cavalry is interesting. The only wholly cavalry regiment, the *ala*, was placed at Mumrills, accessible to the road leading north from Falkirk, though it might be expected that it would have been placed immediately beside the road north. The 1,000-strong mixed infantry and cavalry unit, the *cohors milliaria equitata*, at Castlecary was on the watershed between the Forth and Clyde basins beside a route along the edge of the hills to Stirling and the north. It may also be noted that Castlecary was special both in the units attested there and in the fact that it has produced the latest dated artefacts on the isthmus in both the first and second centuries (Hartley, 1972: 14, 29). The other regiment containing cavalry, the smaller 500-strong mixed unit, the *cohors quingenaria equitata*, lay at Castlehill beside a break in the hills to the north.

The disposition of the regiments on Hadrian's Wall is also interesting (Map 2). At Stanwix, usually perceived as being at the west end of Hadrian's Wall, but in fact close to the centre of the line as measured from Wallsend to Maryport or beyond, lay the only 1,000-strong cavalry unit in Britain, the *ala milliaria*. Stanwix also lay beside the main road north through Annandale and Clydesdale, the route followed today by the main arterial road into Scotland. The rest of the cavalry (both *alae* and *cohortes equitatae*) was generally grouped round the other road to the north, Dere Street, in the east, where also the open plain of Northumberland

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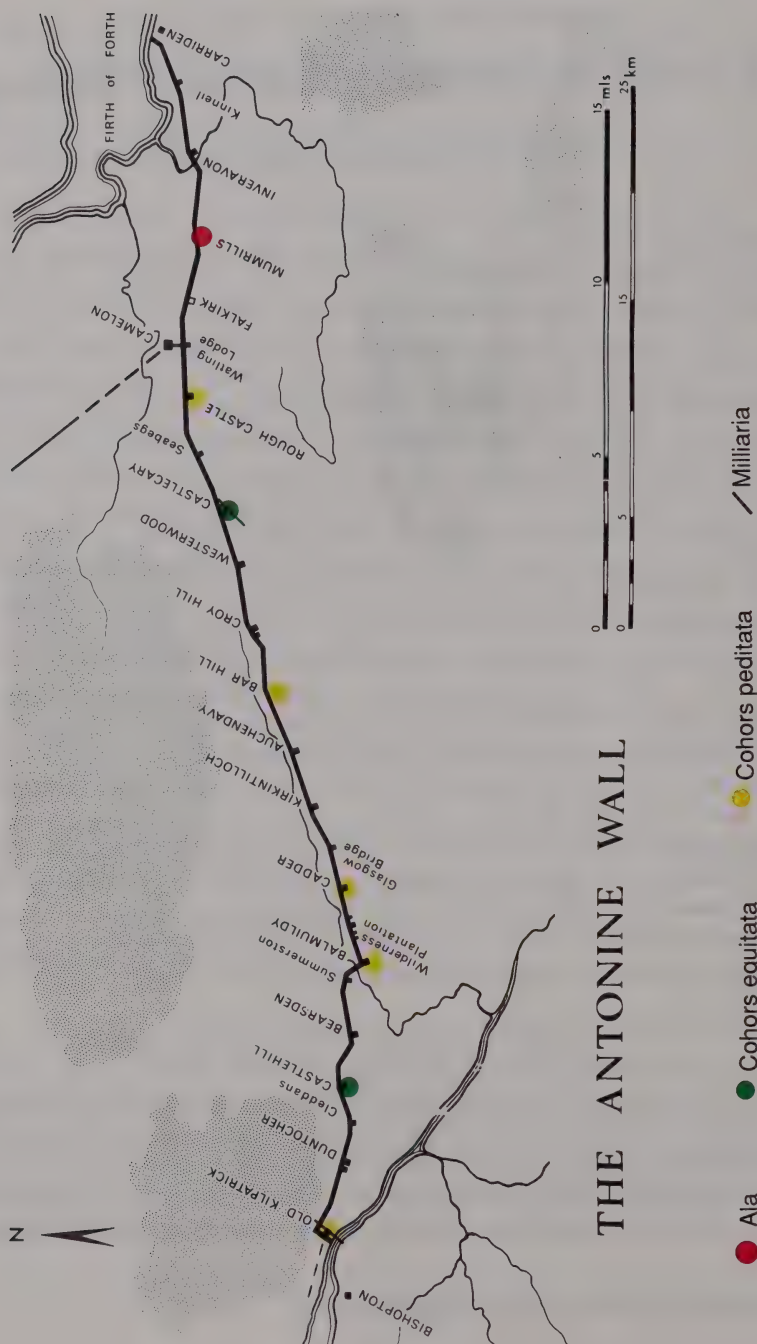
THE ANTONINE WALL

Map 1   Military deployment on the Antonine Wall.
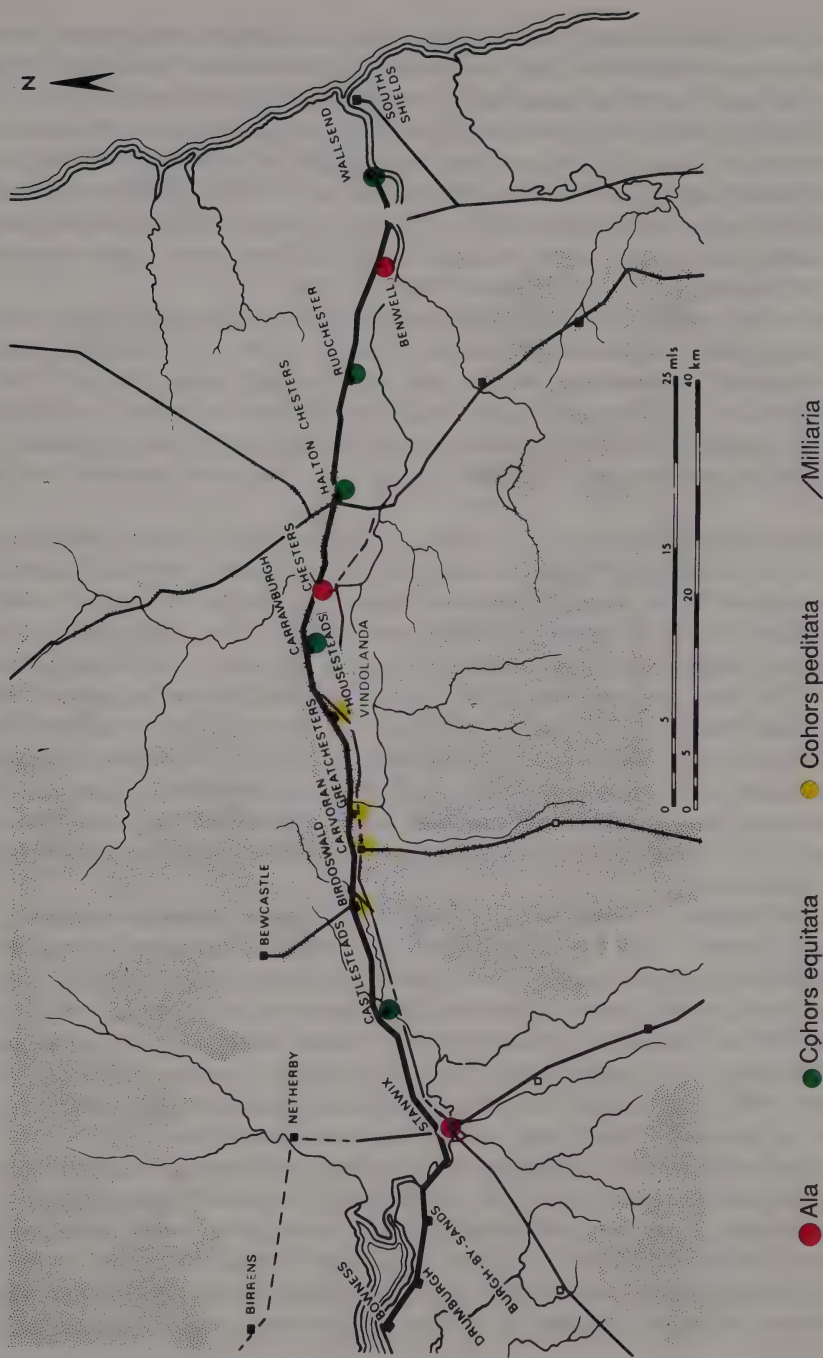
Map 2 Military deployment on Hadrian's Wall in the Hadrianic period.

● Ala   ● Cohors equitata   ● Cohors peditata

● Cohors equitata   ● Cohors peditata

∧ Milliaria

offered good manoeuvrability. The more broken, hilly and perhaps more wooded country of the central sector would be less good for cavalry. This is emphasised by the block of infantry units in the central sector at Housesteads, Greatchesters, Carvoran and Birdoswald. In the third century the cavalry were more evenly distributed, but again the *ala milliaria* at Stanwix was balanced by *alae* in the east, one lying on each side of the Portgate on Dere Street at Halton Chesters and Chesters (Map 3). If we look behind the Wall (Map 4) we can see again the emphasis on the routes leading north, with cavalry grouped behind the frontier along the roads leading to the major crossing points at Stanwix and Dere Street, a point made by Eric Birley 30 years ago (1963: 122; cf. Breeze, 1988: 16–17).

If we turn to a wider field, that of continental Europe, it is immediately obvious that again cavalry regiments were not evenly distributed (Maps 5 and 6). Only along the frontier of Lower Pannonia in modern Hungary were they roughly equally spaced (Mócsy, 1974: 105). Elsewhere there were groupings of cavalry regiments and long sectors with no such units. It might be expected that there was some strategic thinking behind the disposition of the auxiliary regiments along these frontiers, as it has long been appreciated there was for the legions.

There are a number of factors to consider. In examining these, attention will be focused firstly on the distribution of regiments along the European frontier from the Atlantic to the eastern border of Lower Pannonia in modern Yugoslavia during the mid second century, i.e. approximately during the reigns of Hadrian and Antoninus Pius. The frontier in eastern Europe was complicated by the existence of Dacia to the north of the Danube, while the nature of the terrain and the shape of the province of Dacia produced a system of defence corresponding more to that pertaining in Britain than anywhere else in mainland Europe. Perhaps both can most readily be described as 'defence-in-depth', a term that John Mann has used to describe the British system (1979: 145–6; cf. Gudea, 1979: 65). It is inevitable that in some instances the assignment of a particular unit to a particular fort at that time can be challenged, but nevertheless this does not appear to affect the underlying argument and the basic conclusions.

The first factor to be considered is the number and type of regiments available to each provincial governor. 'Each provincial governor' deserves to be emphasised because, so far as can be seen, each governor usually operated independently from his colleagues and this came out very clearly on the frontiers themselves. Governors were not encouraged, as John Mann (1974a: 519) has pointed out, to coordinate their frontier strategies. But in a province like Lower Germany, for example, where there were at least six cavalry regiments stationed (Gechter, 1979: 124), it would have been possible to spread these units evenly along the frontier. As this did not happen, other factors must have played a part.

For long stretches of the frontier, the most usual unit was the smaller mixed infantry and cavalry regiment, the work-horse of the frontier army. This was a unit whose *raison d'être* was the frontier. In war conditions the infantry and cavalry in these units were separated to fight apart, infantry with infantry, cavalry with cavalry (Davies, 1989: 141–51). But otherwise the soldiers of the mixed unit could, and did, operate in mixed patrols and detachments. In the event of more serious trouble the 128 cavalry alone was not an insubstantial force while
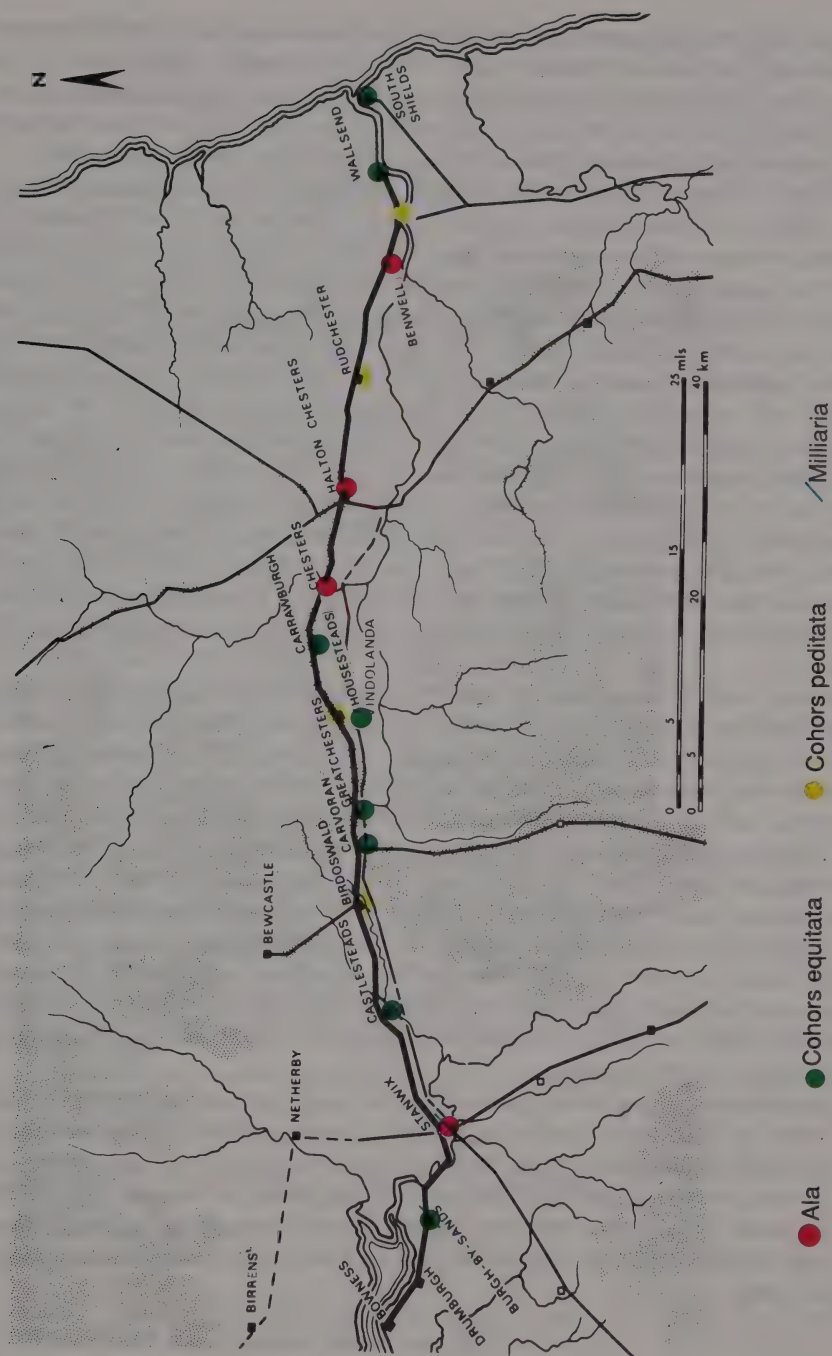
Map 3 Military deployment on Hadrian's Wall in the early third century.

the cavalry from two such units equalled half an *ala*. These units were a flexible response to the day-to-day problems of frontier control where low intensity threats were the main concern. They draw attention to the distinction between the daily frontier control activities of the army and its role in the defence of the empire. These two functions were distinct but become blurred on the frontier as both activities were carried out by the same soldiers.

Yet doubt remains as to how the army actually operated on the frontier. Something of the grand strategy of Roman frontiers is known, such as the alliances and diplomacy, the disposition of troops and the response to attack. Some details are also known: for example, the fact that patrolling took place, that outposts were maintained, that the army imposed regulations on entry into the empire. But what happened on a routine basis is less clear. How often did the patrols take place and how large were they? Were major units such as cavalry regiments employed in these humdrum activities and, if so, did they undertake combined operations with adjacent infantry soldiers? Or were the cavalry employed on different, perhaps more wide-ranging duties, such as long-distance patrolling and surveillance? Certainly, if there was a patrolled wall-walk along the top of Hadrian's Wall, it would seem unlikely that this duty was undertaken by dismounted cavalry (Dobson, 1986: 6), thus arguing for coordination of the activities between the cavalry and infantry based in the forts of Hadrian's Wall. But generally it is difficult if not impossible to answer these questions. Nevertheless, the distribution of the units themselves may help us reach towards a solution. In the face of the partial evidence which survives, it is certainly necessary to use all the available material.

The second factor in considering the reasons for the location of the cavalry units is terrain. This has already been touched upon in relation to the Antonine Wall and to Hadrian's Wall. It can be seen again on the Danube. The three cavalry regiments of the province of Noricum (modern Austria) were located where, in the words of Alföldy (1974: 146), 'there were no obstacles to...rapid movement on either side of the Danube, around Linz with its cavalry fort Lentia, and in the flat Tullnerfeld with regiments stationed at Augustiana and Commagena.' Further west the Danube flows through a gorge which provides a natural barrier to passage, with few soldiers placed here, and where, in all cases, the regiments were infantry or mixed. Much of the frontier here faced forest and this was reflected by the light screen of forts. But at the eastern end of the province a broad plain to the north offered easy approach to the empire and here were five closely spaced forts, two held by cavalry regiments, two by mixed units of infantry and cavalry, the fifth by a thousand-strong cohort, with the legion at Vienna just into the next province, Upper Pannonia.

In Lower Germany it might be considered that the low-lying, wet country of the estuaries of the Rhine and Maas would not be good cavalry country and indeed there was only one cavalry regiment based within a close density of forts (Gechter, 1979: 124). Roman statements survive describing cavalry on horses swimming rivers, but marshes are a different matter for horses. Here, as elsewhere, the role of the fleet should not be forgotten (Margaret Roxan pers. comm.). The river fleets would have provided mobile forces to complement the land-based army.
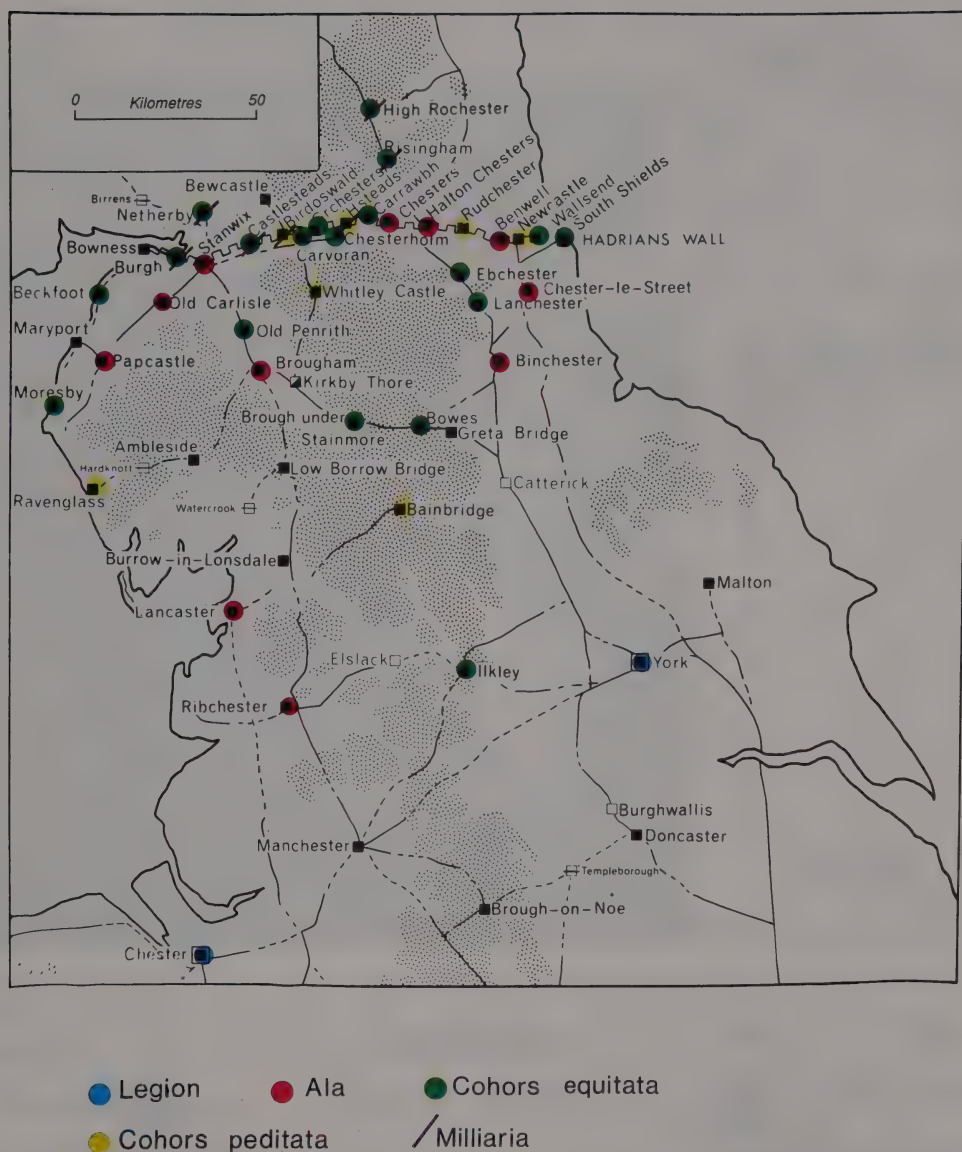
Map 4   Military deployment in northern Britain about 220.
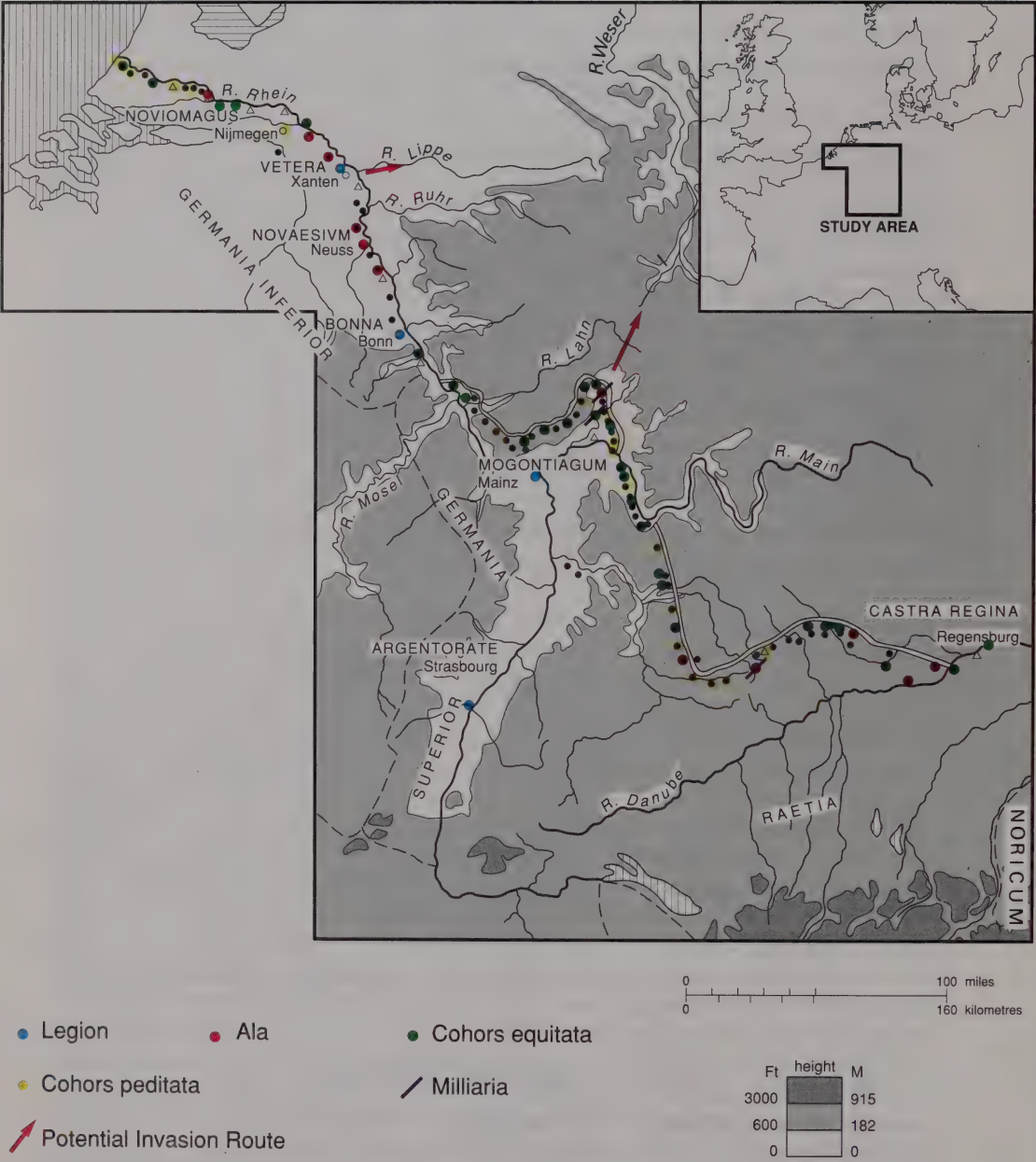
Map 5 Military deployment in Germania Inferior, Germania Superior and Raetia in the mid second century.
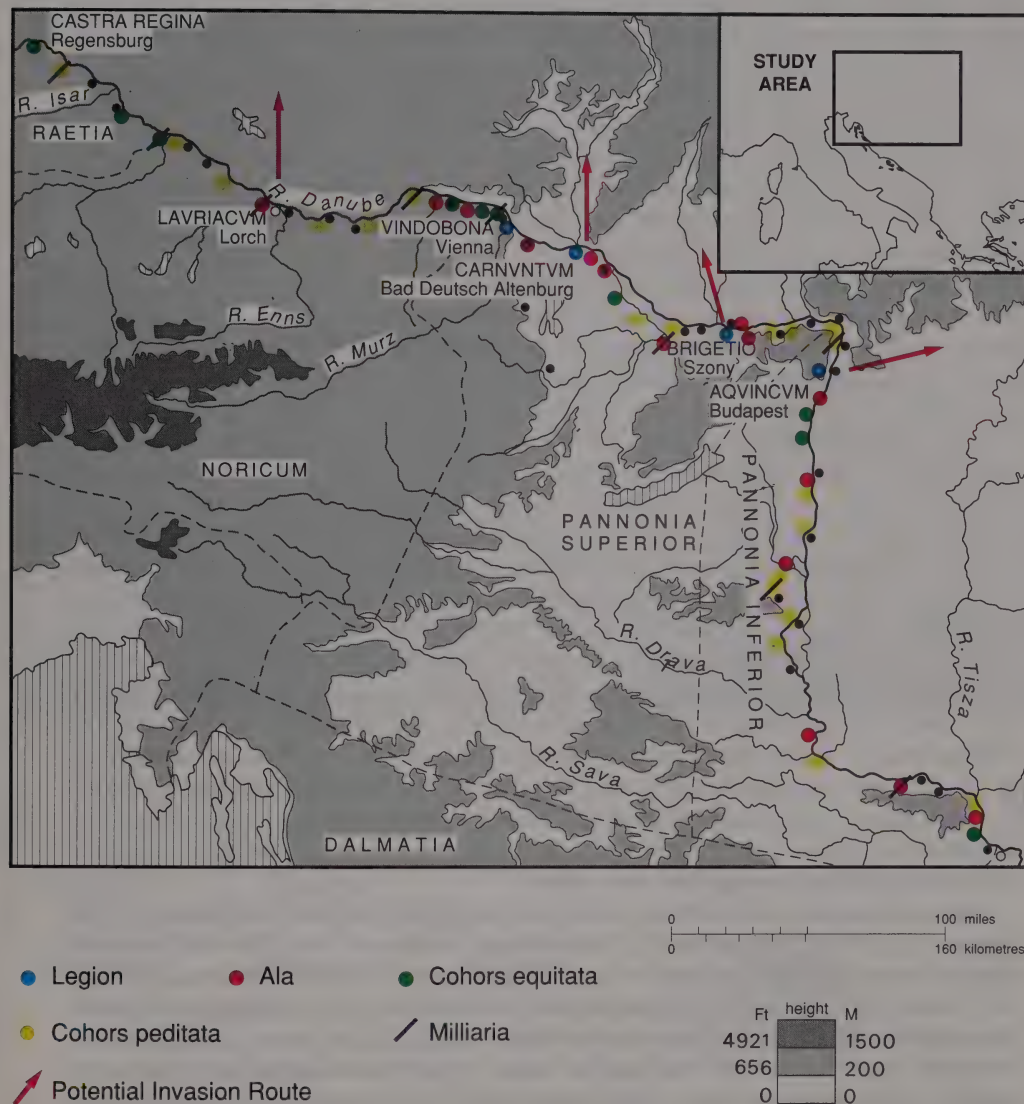
Map 6   Military deployment in Noricum, Pannonia Superior and Pannonia Inferior in the mid second century.

The other cavalry regiments of Lower Germany, five in number, lay up river to the north and south of Vetera (Map 5). This was a legionary base but was also on a major route into free Germany, up the river Lippe. Here may be one reason for the concentration of cavalry units. But a second reason may lie in the terrain, for these units lay in that stretch of country between the low-lying estuarine lands to the north and the hills to the south. It is noteworthy that cavalry units were very rarely located along the frontier which ran through those hills until the more open ground of the Danube valley is reached.

The frontier of Lower Pannonia looked out across the Great Hungarian Plain, the home of the Sarmatians (Map 6). Here was the only regular spacing of cavalry regiments along the whole of the western half of the European frontier. It might be suspected that the roughly regular spacing of cavalry units here related to the monotony of the land beyond. Indeed Mócsy (1974: 105) has suggested that the regular spacing of forts 14 miles apart does reflect the nature of the country beyond the frontier.

The third and fourth factors have already been mentioned: the grouping of units beside a line of advance and the control of routes. It has long been recognised that the legions were placed in relation to potential invasion routes (Maxfield, 1987: 143, 174–5; Wheeler, 1954: 26–34). Thus Vetera lay at the beginning of one route into free Germany, that up the Lippe, while Mainz lay on another (Map 5). Further east Brigetio was at the beginning of one invasion route into Bohemia, Carnuntum a second (Map 6). Carnuntum also lay on the amber route from the Baltic, indicating that it was not just lines of military penetration which dictated the location of legions. Aquincum was placed astride a major road from Pannonia across the northern edge of the Great Hungarian Plain into Sarmatia. When a legion was not available to control a route, a cavalry unit might be substituted, as occurred at Aalen in Raetia and at Linz in Noricum: it may be noted that when a legion was established in Noricum it was placed at Lauriacum, just a few miles from Linz. In Britain the cavalry and part cavalry units were grouped around the two roads leading north, as has been shown earlier.

It is worth noting how often the legions were supported by cavalry. Attention has already been drawn to the grouping of five *alae* around Vetera. A similar situation occured on the Danube. Brigetio was flanked in the east by one cavalry unit and in the north, across the river, by a second. At Carnuntum the fort of *ala I Thracum* has been recently discovered near to the legionary fortress (Stiglitz, 1986). Such groupings of units would have created formidable mixed forces of infantry and cavalry able to strike into free Germany or defend the province from attack by forces coming down these routes.

The cavalry regiment at Echzell lay at that point where the main route from Mainz into free Germany crossed the frontier (Map 5). It was the only cavalry unit in the 300-mile frontier of Upper Germany and was supported by a small infantry unit which occupied the same site and the 1,000-strong mixed unit at Friedberg in the Taunus, the only fort remaining in occupation behind the frontier and itself on the route from Mainz through Echzell and beyond (Oldenstein-Pferdehirt, 1983: 337–41). At first sight the *ala* at Echzell was doing no more than act in support of the legion at Mainz in the same manner as its compatriots at Brigetio and Carnuntum. But the geographical separation of the *ala* from the legion and its position on the frontier leads on to another possibility. Is it possible to see here a hint at different functions
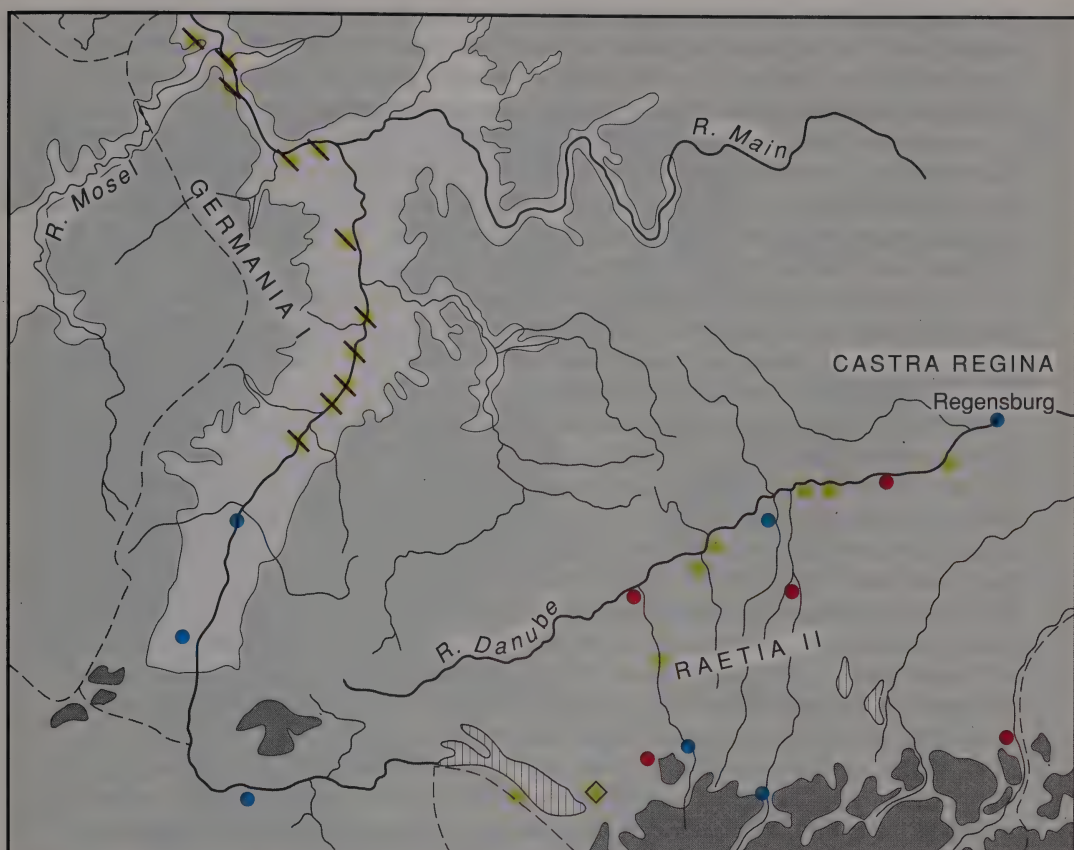
Map 7   Military deployment in the provinces of Germania I, Maxima Sequanorum and Raetia II according to the *Notitia Dignitatum*.

for cavalry regiments on the one hand and infantry or mixed units on the other, a point already touched on? Was the cavalry regiment at Echzell concerned with patrolling further up the route leading into Germany, or undertaking surveillance within the heart of free Germany, leaving the infantry and mixed units to undertake the local patrolling?

A somewhat different reason, namely internal communication, may have governed the location of the other cavalry regiment based in Upper Germany, that at Welzheim on the outer *limes* of Upper Germany, as Dr Barbara Pferdehirt has suggested to me. The *ala* here was probably charged with the duty of protecting the roads linking the Rhineland, and in particular Mainz and Strasburg, with the Danubian provinces.

It is difficult to know how far the army might have wished to control trade routes, but the possibility ought to be considered, especially where it seems that the army might rely upon the importation of food from beyond the frontier (Whittaker, 1983: 115): these would certainly have been important routes to protect.

The final factor which might have affected the placing of units on frontiers was the location of the perceived threat from the other side. This factor is in many ways the most difficult to evaluate. To return to northern Britain, the greater weight of troops on both Hadrian's Wall and the Antonine Wall lay facing those areas which have produced least evidence for the presence of native peoples. These were precisely the areas over which the army needed to keep watch and ward (Breeze, 1985). Elsewhere were farming communities, governed by their own chieftains, which would probably have remained stable under the watchful eye of Rome. These areas could be held on a light rein because of a ready appreciation of the might of Rome; actions against troublesome villagers are described in the pages of Caesar and illustrated on Trajan's Column and the Marcus Column. Chieftains would have advised the army of the presence of dissidents, if only to ensure their own safety.

In contrast, in Noricum, a combination of geography, terrain, thick forest and a lack of people north of the Danube led to a lightly held frontier from the western boundary at Passau to Krems (Map 6). The more open country beyond the eastern end of the province was densely populated and Rome had strong trading relations with this area. But it was an easy point of access from beyond the frontier and this potential threat also helps to account for the strength of the army units in that sector: indeed during the invasion of Noricum by the Germans in about 170 at least two of the forts in the Tullnerfeld were destroyed (Alföldy, 1974: 153).

On the Lower Rhine, the density of forts between Nijmegan and the sea emphasises the threat posed by the Frisians beyond the frontier (Map 5). Yet, as has been noted, there was a lack of cavalry units here, to be explained most readily by the nature of the terrain. Further south, the concentration of three cavalry regiments up river from the legionary fortress at Vetera may be more complicated than either suggestion so far offered, i.e. the importance of the route up the river Lippe and the nature of the ground. The potential threat from the substantial German population in the valleys of the Lippe, Ruhr and Wupper may have been equally, or more, instrumental in determining the necessity for a strong force here (Gechter, 1979: 127). But the threat alone did not dictate the nature of the opposing Roman force, as more cavalry facing the Frisians might have been expected, had this been the sole reason. The
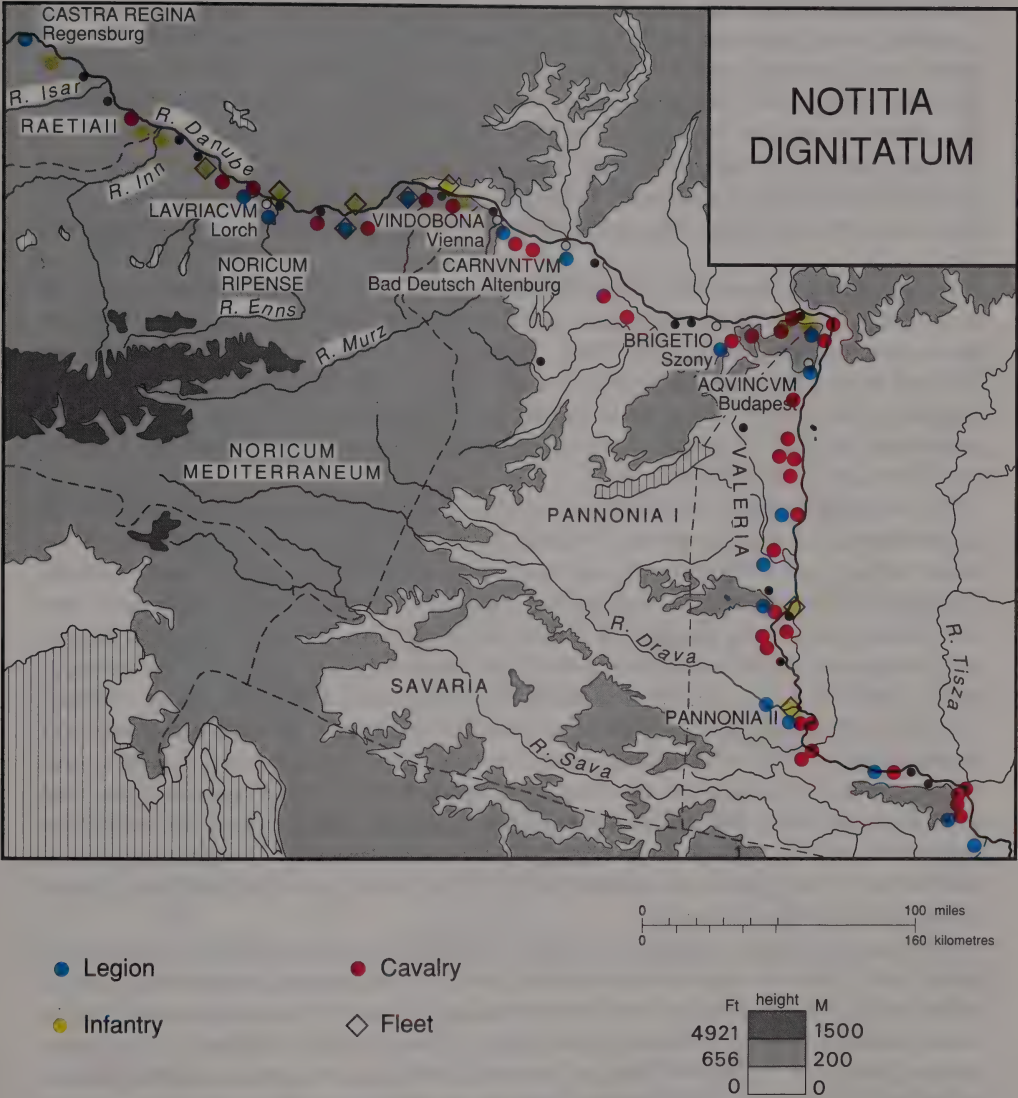
Map 8   Military deployment in the provinces of Noricum Ripense, Pannonia I, Valeria and Pannonia II according to the *Notitia Dignitatum*.

rarity of cavalry in the Rhine estuary demonstrates that several factors operating together probably determined the nature and scale of the Roman forces on any particular stretch of the frontier.

In conclusion, it can be said that there does seem to be sense in the disposition of auxiliary regiments, and in particular cavalry on the Roman frontiers in the second century. Terrain, routes, lines of advance, potential threats and the nature of the forces available all played their part. The supply of the cavalry regiments was presumably also a factor, but as the Roman army always seemed to be prepared to place units in inhospitable places if it suited its purposes, and bring in supplies, this aspect has been placed to one side. Today it is not always clear which factors were paramount in determining the locations of cavalry regiments. No doubt the factors were inter-twined and it will be a difficult, but intriguing, task to disentangle them.

This problem gets even worse in the late fourth century. Maps 7–9 indicate the distribution of units named in the *Notitia Dignitatum* for the same stretches of the frontier as Maps 4–6 which relate to the second century. There is little that can be said about the former Upper Germany as the title *milites* given to all the units here is non-specific (Map 7). Further, the alteration in the line of the frontier to the Danube–Iller–Rhine line (Garbsch, 1970) renders comparisons difficult. We may note, however, that in the former Raetia the number of cavalry units is the same in both periods.

On the middle Danube comparisons are possible (Map 8). The two crucial changes were the proliferation of legions, though obviously of a very different nature than those of the second century, and the considerable increase in the number of cavalry units on the frontier. The number of cavalry units in Noricum was doubled from three to six, but these units were now spread evenly along the frontier (Mócsy, 1974: 202–3). The most striking change is along that stretch of the Danube facing the Great Hungarian Plain. Here also 'the system of strong-points and forts…become[s] substantially denser during the fourth century' (Mócsy, 1974: 284). There was, too, a change in the nature of the forces. The distribution pattern is dominated by infantry in the second century, but cavalry in the late fourth. Lugio, always important (ibid. 101), was now the focus for a strong force guarding one end of the road across the plain. Other than this, though, the former concentration on routes seemed to have disappeared and it is difficult to make much sense of the distribution pattern other than to state that there was a more even spread of units. These frontier regiments could be supported by the field armies in emergencies.

In Britain the picture is different both from the middle Danube and from earlier years in Britain itself. While the cavalry units survived, the former mixed units are now believed to be infantry only. Thus infantry units predominated along Hadrian's Wall where the distribution pattern is frozen from two centuries earlier (Map 9). The new units introduced in the fourth century (Mann, 1974b: 38) were nearly all infantry, giving support to the overall new pattern. Superficially it would suggest differences in the type of threat to the Empire in Britain and on the middle Danube requiring a stronger cavalry—and therefore mobile?—presence in the latter area.
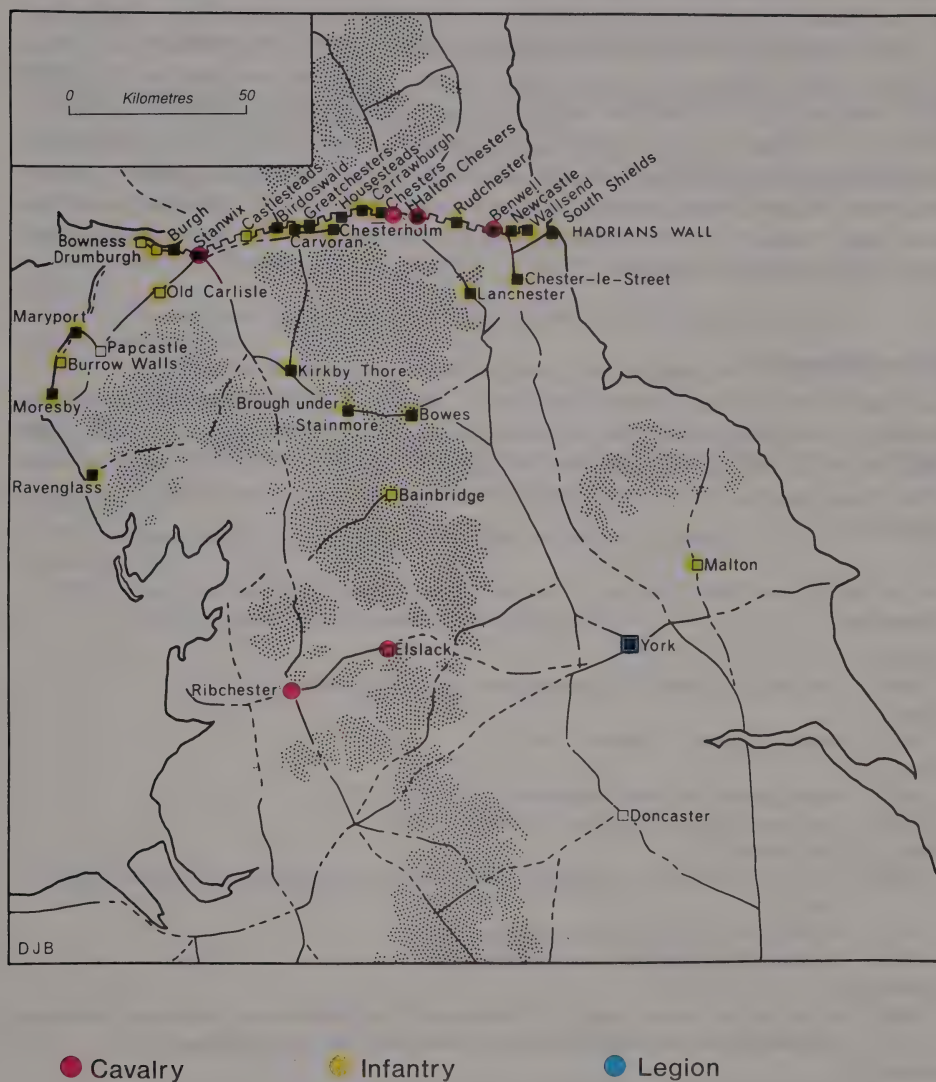
Map 9 Military deployment in northern Britain according to the *Notitia Dignitatum*.

Abstract

The uneven distribution of cavalry, both alae *and* cohortes equitatae, *along the European frontiers of the Roman Empire in the second century, is examined and compared with that of the Late Empire. Although partly a matter of suitability of terrain, other factors operated in the earlier period. Cavalry was sited to control routes and lines of advance, as well as important trade routes and potential threats. In some cases* alae *were sited near legionary bases, thus creating a formidable mixture of heavy infantry and cavalry forces both for offensive and defensive action. In the fourth century the pattern reflects the changed character of the perceived threat. There was a considerable increase in the number of cavalry units on the Middle Danube and they were more evenly spread. In Britain infantry predominated along Hadrian's Wall, suggesting that here the nature of the external danger was different. More research is needed.*

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Persecutors and Martyrs in Tertullian's Africa

by A.R. BIRLEY*

I.

That 'ecclesiastical history is too important to be left to ecclesiastical historians' perhaps no longer needs repeating.[1] Or, to put it the other way round, the Christian sources are simply too interesting to be ignored by the ancient historian. Not that they have been, of course. The activity of the churches from Constantine onwards inevitably forms a major component in the investigation of late Antiquity. And the question, 'Why were the early Christians persecuted?'[2] is a standard topic of mainstream ancient history—major attention going to famous passages in Pliny, Tacitus, and Suetonius. What happened between Pliny's encounter with Christians in Pontus *c.* 111 and the Great Persecution under Diocletian needs further attention; and enquiry can be profitable. There are a few 'authentic' *Acta Martyrum* and there is the copious patristic literature. Further, some of the persecutors are otherwise recorded. This paper is intended simply as an account of persecution in North Africa during the years 180–212, with emphasis on the identity and background of the persecuting governors. It springs from a longstanding fascination with the writings of Tertullian, with Roman North Africa, and with the Antonine-Severan era.[3] Not exactly 'late antiquity', it is true, which is the theme of this colloquium. But the scholar in whose honour this paper is offered and his mentors and fellow-students at U.C.L. (to all of whom the present writer is much indebted) cast a broad compass.[4] It is to be hoped that John Mann will find items of interest in the pages that follow.

II.

'Vigellius Saturninus, who was the first here to use the sword against us, lost his sight.' Thus Tertullian opens the brief rubric in the *Ad Scapulam* of Roman officials who suffered for having persecuted the Christians.[5] By good fortune we have more details of this case from a contemporary source, the *Passion of the Scillitan Saints* [6]

> Praesens, for the second time, and Condianus being consuls, the 17th day before the Kalends of August [16 July 180], at Carthage, in the *secretarium* (council-chamber), Speratus, Nartzalus, and Cittinus, Donata, Secunda, Vestia, having been brought in, Saturninus the proconsul said: 'You can earn the pardon of our Lord the Emperor if you return to your right mind.'

> There follows a dialogue, no doubt abbreviated. First an exchange with Speratus, then a

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curt appeal from the proconsul to the others:

> 'Do not join in this madness', to which separate replies from Cittinus, Donata, Vestia, and
> Secunda are given. Saturninus then resumed his interrogation of Speratus, and offered a
> thirty day adjournment for reconsideration. It was refused. The proconsul then read out his
> verdict from a writing-tablet: 'It is decided that Speratus, Nartzalus, Cittinus, Donata,
> Vestia, Secunda, and the rest, having confessed that they live according to the Christian
> rite, since, when the chance of returning to the way of the Romans was offered them, they
> obstinately persevered, should be punished by the sword.'

No inscription records Vigellius' presence in Africa, but the man is otherwise known. At
Troesmis on the Danube, in Lower Moesia, the town council voted a statue in his honour
when he was 'legate of the emperor', that is, legate of the legion V Macedonica, then still
garrisoned there. His command may be dated to the late 150s—before long, the legion left
Troesmis for the Parthian war, after which it was transferred to Dacia.[7] A little later Vigellius
turns up in southern Asia Minor, as governor of Lycia-Pamphylia. His names occur on a
fragmentary inscription at Pamphylian Side, near the eastern end of his twin province, and a
statue base in the theatre of opulent Perge honours him as 'pro-praetorian legate of the
emperor, the most distinguished governor of Pamphylia and Lycia...patron and benefactor of
the city'.[8] He probably held the post *c*. 160–3 and must have reached the consulship soon
after. It is likely enough that he went on to govern other provinces in the later 160s and 170s,
but as yet details are lacking.[9] The *Passion* calls him just 'Saturninus', Tertullian gives his
gentilicium 'Vigellius' as well, as does the Perge dedication, which also supplies the *praenomen*
'P(ublius)'. At Troesmis and at Side he was honoured under his full nomenclature: 'P. Vigellius
Raius Plarius Saturninus Atilius Braduanus Caucidius Tertullus'. In the Antonine fashion he
had added a string of extra names to his original ones.[10] He shared the last four with kinsmen
of Herodes Atticus' wife, the Atilii of Libarna in Cisalpine Italy, who had provided senators
for several generations.[11] 'Plarius' has been supposed to indicate connections with Rhodes,
but in the Antonine period it points to Ostia, where there was a family of Plarii intermarried
with senators. 'Raius' is uncommon, but again suggests Italy, as does 'Vigellius' itself.[12]

Vigellius was probably born in the 120s, so would have been in his mid-fifties when he
came to Carthage as proconsul in the spring of 180, shortly after the death of Marcus
Aurelius. Africa had been sheltered from the upheavals that afflicted the rest of the Empire
during Marcus' reign: foreign wars in the east and the north, with even Italy herself and
Greece invaded by barbarians, disturbances in Britain, Moorish incursions into Spain; plague;
rebellion in Egypt; a usurpation, and almost civil war, in the east.[13] Africa's prosperity and
self-confidence had reached new heights. Its Christian community went back a long way, no
doubt. Exactly when and how the faith arrived remains unclear.[14] The martyrs of Scilli,
Africa's earliest firmly dated Christians, are variously instructive. Their home and their very
names are telling. Scilli (if that was the exact name of the place) was a small town at best,
probably close to the great metropolis, Carthage.[15] The main part of the *Passion* names six
persons: three men, Speratus, the main spokesman, Cittinus, and Nartzalus; and three women,
Donata, Vestia, and Secunda. The list read out by the proconsul's herald adds six more: four

men, Veturius, Felix, Aquilinus, Laetantius; and two women, Januaria and Generosa.[16] Two of the twelve names are very characteristic of Roman Africa, past participles which translate Punic originals, Speratus and Donatus. Felix and Januarius are also much favoured in Africa, although common everywhere. Cittinus and Nartzalus are 'native', that is, Numidian or 'Berber'. Secunda, Veturius, and Aquilinus are relatively standard Latin names, Veturius a *gentilicium*, the others *cognomina*. Generosa and Laetantius look like specifically Christian formations, the latter being otherwise unattested; and Vestia is also unknown elsewhere.[17] Taken as a whole, the names suggest a Christian community which included more than one social group. But, as the last sentence of the *Passion* reveals, they must all have been Roman citizens, since they were beheaded, the simplest and quickest form of execution, normally reserved for citizens.[18]

Vigellius refrained from any attempt to coerce the twelve by torture, soon to be a regular feature of these proceedings in Africa, and recently experienced by the martyrs of Lugdunum.[19] His line of questioning deserves examination. He began, as already seen, by offering pardon, on the emperor's behalf, for a return 'to a right mind', or 'right way of thinking' *(bona mens)*—the contrast is with the Christians' madness or mindlessness *(amentia* or *dementia)*, a term Pliny had used.[20] Speratus' reply was directed to the accusation of criminality, implied but not spelled out:

> 'We have never done wrong, we have given no service to wickedness, we have never cursed. Rather, when ill-treated we have given thanks, because we honour our Emperor.'
>
> Saturninus: 'We too are religious and our religion is simple and straightforward; and we swear by the *genius* of our Lord the Emperor and we pray for his health, which you too should do.'
>
> Speratus: 'If you offer to listen calmly, I can tell of a simple and straightforward religious doctrine.'
>
> Saturninus: 'If you begin to say bad things about our sacred rites I shall *not* listen. Rather: swear by the *genius* of our Lord the Emperor.'
>
> Speratus: 'I do not recognise the empire of this world. Rather do I serve that God whom no human has seen nor can see with these eyes. I have not committed theft. But whenever I buy anything I pay the sales tax, since I recognise my Lord the Emperor, ruler over kings and over all nations.'[21]

The original exchanges may well have been lengthier. Here the writer was no doubt concerned to stress the Christians' loyalty. What also comes across in the first part of the *Passion* is the proconsul's view of proper conduct, 'right mind', and the notion that the Roman way was simple and straightforward (*simplex*). Speratus seized on this term: *he* can expound a truly *simplex* religion. Saturninus would not be deflected, refusing to listen to 'insults about our sacred rites'. He may well have had some acquaintance with the by now standard repertoire of Christian Apologists, including sustained mockery of pagan cult and the traditional deities.[22] The recommendation to swear by the Emperor's *genius*, his tutelary divinity, was made into an order. Speratus' response that he did not recognise 'the empire of

this world' was apparently at odds with the claim he had just made that 'we honour our Emperor'; which indeed he repeated even more fully after proclaiming his allegiance to the unseen God and his innocence of theft and of tax-evasion. It is not surprising that the governor evidently lost patience with Speratus and addressed the other accused:

'Cease to be of this persuasion.'

Speratus, although not spoken to, nonetheless replied first: 'It is an evil persuasion to commit murder, to bear false witness.'

Saturninus again directed his remarks to the others: 'Do not share in his madness', *dementia,* the opposite of *bona mens.*

Four of Speratus' companions now spoke in turn:

Cittinus: 'We have none other whom we may fear except our Lord God who is in the heavens.'

Donata: 'Honour Caesar as Caesar; but fear God.'

Vestia: 'I am a Christian.'

Secunda: 'What I am is what I want to be.'

Saturninus then questioned Speratus again: 'Do you persist in being a Christian?'

Speratus said: 'I am a Christian', and all agreed with him.

Saturninus now offered an adjournment, his language showing that he expected the offer to be rejected: 'I don't suppose you want an interval to reflect?'[23]

When Speratus and the others duly declined, Saturninus suddenly asked a further question: 'What have you got in your case?'

Speratus: 'Books and letters of Paulus, a just man.'

The proconsul's reaction is not registered. He might perhaps have been aware of the apostle's identity, that Paul had been an important figure in the origins of Christianity. He will hardly have known that Paul had been at Perge, where his own statue stood in the theatre.[24] Speratus' statement is generally taken as evidence that a Latin translation of at least *Acts* and the *Epistles* of Paul existed at this date, although it is not impossible that Speratus could read Greek.[25] At any rate, Saturninus now simply decided to cut short the hearing: 'You have an adjournment for thirty days; and think it over.' But Speratus merely repeated his previous answer ('I am a Christian') and once again the others signified their agreement. The *Passion* registers no adjournment following this, only Saturninus' verdict, read from a tablet.

Speratus said: 'We give thanks to God.' Nartzalus said: 'Today we are martyrs in the heavens. Thanks be to God.' Saturninus had the herald read out the names of the twelve, to be led off for immediate execution. They all said: 'Thanks be to God', and were at once beheaded for the name of Christ. Amen.[26]

III.

A few years after the deaths of the Scillitani, a highly educated and passionate native of Carthage, Q. Septimius Florens Tertullianus, became a Christian.[27] With Tertullian, Latin

Christianity found a voice. His earliest datable writings belong to the later 190s, the latest with a clear date to 212 or 213.[28] Three of his first pieces, the *Ad nationes* of 197 and the *Apologeticum* which replaced it the following year, and the *Ad martyras*, are directly occasioned by persecution. So too was the latest work, the letter to the proconsul Scapula, in office from 212 to 213.[29] In between came several other pieces in which persecution was a major theme: the *Scorpiace*, warning the faithful of heretics who sought to evade martyrdom; the *De fuga in persecutione*, sternly instructing his 'brother' Fabius that Christians must not flee; and the *De corona*, inspired by the trial of a Christian soldier, on trial for refusing to wear a garland at a ceremony to receive imperial bounty, probably in 211.[30]

Persecution in the years from 197 to 198 might well have been sparked, in North Africa not least, by the purges which followed the defeat of Severus' rival Clodius Albinus at Lugdunum in February 197. Albinus came from Hadrumetum (Sousse) in proconsular Africa and had numerous supporters there.[31] It is no surprise to find imperial procurators in Africa specially appointed to 'confiscate the property of the condemned' or to administer it.[32] At the end of the *Ad martyras* Tertullian alludes to the 'unexpected fate' of distinguished persons for supporting the wrong side, as evidence 'from the present time of how people may suffer'.[33] In the *Ad nationes*, passionately defending Christians against the charge of being treasonable, he evokes 'Roman loyalty to the Caesars': 'The Syrian provinces still reek with the stench of corpses, the Gallic provinces still do not wash in their own Rhône'—it was filled with the bodies of Albinus' supporters.[34] The theme is expanded in the *Apologeticum*: the supporters of Cassius (Marcus Aurelius' rival), Niger, and Albinus were not Christians. 'But to come to those who now are daily revealed as accomplices in criminal plots or their supporters (the gleanings left after a harvest of assassins)'—these very people had displayed their so-called loyalty by the ostentatious decoration of their houses, in which Christians declined to join. He goes on to refer to 'those who consult astrologers and soothsayers and augurs and magicians about the life of the Caesars'.[35] The Augustan History *Life of Severus* supplies the exact context. In summer 197 Severus, who had already purged Albinus' supporters at Rome and in the west, went to Syria to prepare for his second Parthian campaign. 'In the meantime…he hunted down the remnants of Pescennius Niger's following, to the extent that he even laid hold on some of his own friends as conspirators against his life. He also killed many for allegedly consulting astrologers or seers about his health.'[36]

The 'martyrs' to whom Tertullian wrote were in fact 'blessed martyrs designate', awaiting their ordeal. 'The flesh perhaps will fear the heavy sword and the lofty cross and the fury of the beasts and the supreme penalty of burning and all the executioner's craft in torturing', but the spirit must set against this the knowledge that many have endured, and have even sought this glory.[37] In the *Ad nationes* Tertullian for the first time addressed the heathen: the 'nations' are, in Latin translation, the Gentiles of the Old Testament. He had clearly been reading both Pliny's correspondence about Christians with Trajan and Tacitus' account in the *Annals* of the execution of Christians after the Great Fire of 64. To some extent the *Ad nationes*—and its more polished replacement the *Apologeticum*—are sustained replies to Pliny and Tacitus. Above all, there recurs again and again Tertullian's mocking denunciation of hatred based on ignorance: Tacitus stressed how the Christians were hated, Pliny opened

his letter to Trajan by confessing his own ignorance. The language of Pliny and Tacitus recurs repeatedly throughout these two works and in the *Ad Scapulam*.[38]

Tertullian mocks the attempt to produce recantation by torture: 'you press the *guilty*, by torture, to *confess*, if they refuse to admit their guilt—yet Christians, who have freely confessed, you try to compel by torture to *deny*. What great perversity!'[39] Not long after this he recast the treatise. The new work, the *Apologeticum*, was addressed directly to 'You, the overseers of the Roman empire, set on high in the light of day in the very summit of the city'—on Carthage's acropolis, the Byrsa—'presiding to do justice, publicly to examine, face to face.' Yet, in a recent 'family case' ('quod proxime accidit, domesticis iudiciis') investigation of Christianity had been blocked. Tertullian proposes to fill that gap.[40] Clearly, the previous proconsul had declined to hear further details: it was enough, as it had been for over a century, if the accused was or was not a Christian.[41] Tertullian quotes the widespread popular belief that Christians were incestuous and cannibalistic: Pliny had found nothing to support this—Trajan had actually ruled that Christians were not to be hunted down—yet still must be punished if accused.[42] The use of torture to produce recantation is a contradiction. 'This empire, of which you are ministers, is the rule of citizens, not a tyranny.' He reproaches the enemies of Christianity with infanticide: 'how many of those spectators who pant for the blood of Christians, how many of you governors, so just and so severe against us Christians, should I touch in your own consciences for killing your own children?' He reels off the punishments Christians suffer: 'You put Christians on crosses and stakes…you tear the sides of Christians with hooks…we lay down our necks…we are driven to the beasts…we are burned by flames…we are sentenced to the mines…we are banished to islands.'[43]

After an exposition of the origins of Christianity and an assault on pagan cult, he produces a ringing defence of religious freedom: 'Look to it: may this too not form part of the accusation of irreligion, to abolish freedom of religion and forbid the choice of deity, so that I may not worship whom I wish but am compelled to worship whom I do not wish? No one, not even a man, would wish to be worshipped by an unwilling worshipper.' But Christians will not give in—'when challenged to sacrifice, we stand unmoved in loyalty to our conscience…one thinks it madness (*dementia*) that, although we could for the moment sacrifice and depart unharmed, with a mental reservation, we prefer "obstinacy" [Pliny's word] to safety.' Yet Christians, he stresses, are loyal to the emperors: 'we pray for them long life, secure rule, a safe household, brave armies, a loyal senate, an honest people, a peaceful world.' Nonetheless, if you will, 'go to it, good governors, rack out the soul that prays to God for the Emperor.'[44]

He takes up the charge that Christians do not celebrate imperial festivals: yet they never participate in uprisings and treason—with reference to the current wave of treason trials and purges. Christians are actually told to love their enemies: yet 'how often do you wreak your fury against Christians, partly obeying your own instincts, partly the laws? And how often, aside from you, does the hostile mob on its own account attack us with stones and fire? Mad as Bacchanals, they do not even spare the Christian dead.' Christians are made scapegoats for every form of disaster: 'if the Tiber floods to the walls, or the Nile fails to flood the fields, if the sky stands still or the earth moves, if there is famine, if there is plague, at once the cry is

"Christians to the lion!"'[45] Yet Christians are never found guilty of ordinary crimes, no Christian is ever on the list of temple-robbers, fraudsters or baths-thieves. 'We, then, alone are innocent.' But whatever is done against them,

> We fear God not the proconsul. You may think we are glad to suffer. We are not, but we fight, like soldiers, when we have to. But we are condemned. Yes, when we have achieved our purpose. So we have conquered when we are killed, we escape when we are condemned. You may call us 'firewood-men' and 'half-axle-men' because we are tied to a half-axle-post, the firewood is piled around us and we are burned. This is our garb of victory, our palm-embroidered robe, our triumphal chariot.[46]

He ends on a note of defiance.

> But go to it, good governors! The people will think you the better if you sacrifice Christians to them, torture us, rack us, crush us: your cruelty merely proves our innocence... Lately when you sentenced a Christian woman to the pander (*leno*) rather than the lion (*leo*) you admitted that we count injury to chastity more dreadful than any penalty, any death. Nothing is achieved by your cruelty, each time more elaborate than before: it is the bait that attracts to our school. We multiply whenever we are mown down by you: the blood of Christians is seed...when we are condemned by you, we are acquitted by God.[47]

The vivid language of Tertullian's letters to the martyrs and the 'Nations' and of his *Apology* addressed to persecuting governors, leaves no doubt that Christians were frequently being tried and sentenced, after torture, in the Africa of the late 190s.[48] It is not until a few years later that we have further evidence of particular Christians who were martyred there. But not all governors had paid attention to accusations against Christians. In the *Ad Scapulam*, written in late 212 or early 213, Tertullian gives some specific illustrations.

IV.

After giving three examples (Vigellius being the first) of persecutors who suffered at God's hand for their deeds, he warns Scapula, who had evidently begun to torture Christians.[49] 'We are not trying to frighten you, we who are not afraid,' he goes on. 'But I merely would like to assure the safety of all by warning: "Don't fight against God"'—here he turned to Greek (*me theomachein*). Coercion of confessed Christians was contrary to official guidelines (*mandata*). Besides, he goes on, 'how many governors, whether more steadfast or more cruel, have dissembled in cases of this kind!' He gives four examples:

> Cingius Severus, who at Thysdrus himself supplied a remedy, a way for Christians to reply which allowed them to be released; Vespronius Candidus, who released a Christian as being merely a disturber of the peace, to answer charges in his local community; Asper, who merely had a man mildly punished [presumably a beating is meant] and at once set down, and did not compel him to make sacrifice—having previously avowed, in front of the lawyers and assessors, his regret that he himself had landed in a case of this kind for the first time. Pudens, too, released a Christian who had been sent before him on a written

charge, having realised that extortion was involved: he tore up the charge-sheet and said that, in accordance with his *mandatum* he would not hear a case against a man with no accuser. All these items could be brought to your attention by your staff *officio* and by those same lawyers, who have personally enjoyed benefits from Christians, loudly deny it though they may.[50]

The four lenient governors are all otherwise known. In order of seniority, their consulships may be approximately dated as: Vespronius, *c.* 176, Cingius *c.* 181, Asper *c.* 185, Pudens *c.* 194. Vespronius, by his full names L. Vespronius Candidus Sallustius Sabinianus, was a native of Mantua in northern Italy, the only certainly known senator from that city. He was commander of III Augusta, and thus *de facto* governor of Numidia, *c.* 174–6, and consul while still there; he was governor of the Three Dacias early in the reign of Commodus, attested by an inscription datable between 183 and 185; and in May 193, as Severus' expeditionary force marched south towards Rome, he was one of the senatorial envoys sent by Didius Julianus in a vain attempt to halt the invasion. Indeed, he is singled out for mention. In a fragment of Cassius Dio he is described as 'in rank very senior, but much more prominent for his sullenness and lack of culture'; and he was nearly killed by Severus' troops. The *Historia Augusta* supplies a reason: the old ex-consul 'was hated by the men from long since for his strictness and meanness.' Vespronius could no doubt have been proconsul of Africa shortly before 193; but it is not unlikely that he released a Christian in his capacity as legate of Numidia. (In that case, it may be noted, the earliest record of Christians in North Africa is pushed back a few years before 180.)[51]

Cingius Severus is less well attested. Apart from the proconsulship of Africa, known only from this passage of Tertullian, an inscription of the year 183 shows him serving as curator of sacred buildings at Rome. This office was normally held by recent ex-consuls; we may guess that he was consul *c.* 181 and proconsul *c.* 195–6. In the meantime he had earned brief prominence, at the meeting of the Senate in the night of 31 December 192, when the news of Commodus' assassination and the elevation of Pertinax as emperor was announced; it was hailed with impassioned acclamations. But later in the meeting, one of the consuls designate, Fabius Cilo, was discovered to have had Commodus' body buried. There was a general outcry of rage. Cingius then spoke:

> Wrongly was he buried. I speak as *pontifex*; the college of pontiffs says this... I move that what that man, who lived only for the destruction of citizens and for his own shame, compelled to be decreed in his honour, be abolished; that his statues, which are every-where, be destroyed; that his name be erased from all public and private monuments; and that the months be called by the names by which they were called when that evil first fell upon the republic.

The months, which had all shortly before been renamed after Commodus' own string of names, did indeed revert permanently to their old style. But the other measures against Commodus' memory did not last long: in 195 Severus began to honour him as 'the deified Commodus', and he openly ratified this after his victory in the last civil war in early 197. At the same time he carried out a drastic purge of his opponents, 29 senators being put to death,

according to Dio. Cingius was a further victim, the charge in his case being an attempt to poison Severus.[52]

C. Julius Asper was probably in office *c*. 200. When Tertullian composed the *Ad Scapulam*, his name had become particularly well known: Asper held a second consulship, as *ordinarius*, at the beginning of 212, with his son as his colleague, a remarkable honour. He was, it seems, prefect of Rome at the time. At Tusculum, where the family had an estate, the elder Asper was honoured as an 'outstanding orator and most faithful defender of clients'. Among those who counted him as their *patronus* were the two Mauretanian and all three Spanish provinces. At the end of 212 both father and son fell into disfavour with Caracalla. As Dio put it, Asper, 'by no means to be despised either for his culture or his intelligence', after being exalted by Caracalla, 'was suddenly outrageously insulted and sent back to his native town, with abuse and in terrible fear.' The native town was either Pisidian Antioch, Asper being a descendant of one of the Augustan veterans settled there, or perhaps the Pamphylian port of Attaleia, where he evidently also had property. Asper had good reason to know more about the sect than most senators, for all that he had never had to hear a case against them before. Both these cities had strong and early Christian links.[53]

The last of the lenient governors, C. Valerius Pudens, was in office a year or two before Scapula. His names are so common that his origin is impossible to determine, although it may be suspected that it was fairly humble. What is known of his career suggests a tough military man. The first recorded post is the governorship of Lower Pannonia, where Pudens is attested among other inscriptions by three altars he himself dedicated: 'To the Gods and to the Spirit [*Genius*] of the province Pannonia'; 'to Minerva the Victorious' and 'To the Fortune of this place'. It appears that Pudens was there at a vital moment, as the immediate neighbour and junior colleague of L. Septimius Severus when the latter was proclaimed emperor at Carnuntum on 9 April 193. As a founder member of the winning faction in the ensuing civil wars, it is not surprising that Pudens went on to major military commands, first in Lower Germany, no doubt straight after the battle of Lugdunum. He is on record there ordering the rebuilding of the armoury at a fort near Leyden in 197. A few years later he was promoted to Britain, where an inscription of 205 registers the construction of a barrack block at a Yorkshire fort under his orders. In Africa a stone from the small town of Vazitana Sarra shows Pudens authorising the transfer of the statue of a god (evidently Aesculapius) to a new temple donated by a local magnate. His conduct when a Christian was brought before him was no doubt far from unique. The *mandatum* to which he referred may have been based on Trajan's rescript to Pliny or, perhaps, that of Hadrian to Minicius Fundanus.[54]

V.

In the second half of 202 began a brief episode of special importance for Roman North Africa, an imperial visit. Apart from Hadrian, nearly seventy-five years earlier, no emperor had ever set foot in the African provinces. Now the African emperor, Septimius Severus, arrived, with his Syrian wife Julia Domna, their sons Antoninus (Caracalla) and Geta, the Guard Prefect Fulvius Plautianus and, no doubt, a great retinue. The visit was marked by specific benefits.

Carthage, the metropolis, Utica, the oldest city, and Lepcis Magna, the emperor's birthplace, were singled out by the granting of the 'Italian right' (*ius Italicum*), which put them on a par with the cities of Italy herself.[55] Carthage had won another privilege, the right to hold a new Greek-style festival, the 'Pythicus agon', to which competitors in the various events, both musical and athletic (with emphasis on boxing and all-in fighting), came from all over the Greek half of the Empire. It was a suitable recognition of the strong Hellenic element in Carthage's population.[56]

At Lepcis a magnificent building programme was under way. Artists, sculptors and crafts-men from the eastern Mediterranean, not least from Aphrodisias in Caria, renowned in this sphere, arrived to work on the expensive marble shipped to Tripolitania to adorn the now favoured city.[57] It was surely no coincidence that a Roman knight whose home was at Aphrodisias, P. Aelius Hilarianus, was at this very time holding the senior procuratorship in proconsular Africa. It may be conjectured that his task included the supervision of his fellow Aphrodisians' work at Lepcis.[58] As it turned out, he had to take on higher responsibility. The proconsul for 202–3, Minucius Opimianus, died; Hilarianus was made acting governor in his place.[59] In this capacity he sentenced a small group of Christians to death.

It was probably early in 203, perhaps in January, that a few young Christians were arrested and imprisoned.[60] They are described as 'young catachumens', converts undergoing instruc-tion: two slaves, Revocatus and Felicitas, the latter well into the second half of a pregnancy, Saturninus, Secundulus, and a young woman named Vibia Perpetua, 'aged about twenty-two, with an infant son at her breast'.[61] The Latin text of the *Passio* does not say where Perpetua and her companions came from, although it is commonly assumed that they were from Carthage. However, the Greek version, as well as the Latin *Acta* and later martyrologies, are unanimous in giving their home town as Thuburbo Minus, a Roman *colonia* founded for veterans of the eighth legion in 35 BC, 45 kilometres (27 miles) west of Carthage. There seems no good reason to doubt this information, although it was to Carthage that they were taken after arrest and at Carthage that they were martyred.[62] Perpetua is said to have been 'honour-ably born' ('honeste nata'), which should mean that her father was at least a town councillor,[63] and well educated ('liberaliter instituta'). It seems that the family was Greek: at least, Perpetua spoke and wrote in Greek, and one of her brothers, who had died at the age of seven, had the Greek name Dinocrates.[64] She had two other, surviving brothers, one of them also a Christian catachumen.[65] No husband is mentioned (he was perhaps dead) but her father, mother, and an aunt (mother's sister) are referred to several times in the *Passio*,[66] perhaps the most famous of all the early accounts of martyrdom and certainly among the most authentic. Most of the text is formed by Perpetua's own account of her time in prison, including four dreams or visions, together with the dream of her fellow-martyr Saturus.

Perpetua's father was not Christian, but the rest of the family evidently was. The five listed near the start of the *Passio* were soon to be joined by Saturus, 'who had himself built us up', in other words had converted them, and 'later gave himself up voluntarily for our sake' (he had been away when they were arrested).[67] The *Passio* also mentions four other martyrs, another Saturninus, Quintus, who died in prison (as did Secundulus also), Jucundus, and Artaxius, and another catachumen Rusticus, who helped Perpetua during her final ordeal in

the amphitheatre but was apparently not a martyr himself: two deacons, Tertius and Pomponius; the priest and teacher ('doctor') Aspasius; and the bishop, presumably of Thuburbo, Optatus.[68] It is puzzling, at least at first sight, that the clergy were not also arrested. 'The blessed deacons Tertius and Pomponius', at any rate, cannot have been unknown to the authorities, for they ministered to the group in prison.[69]

While the five were still in the hands of the governor's attendants ('cum prosecutoribus'),[70] who presumably came to arrest them, Perpetua's father visited her and tried without success to make her abandon her Christianity, leaving in frustrated rage. The group was baptised and a few days later was taken to the prison: 'I was terrified', Perpetua wrote, 'because I had never experienced such darkness. What a desperate day that was: stiflingly hot because of the overcrowding, and there was the soldiers' attempted extortion, and finally, I was tormented by worry about my baby being there.' Tertius and Pomponius bribed the soldiers to let them go to a better part of the prison for a few hours to refresh themselves and Perpetua was able to feed the baby, already faint from lack of milk. Her mother and brother were able to visit and Perpetua entrusted the baby to their care. 'These anxieties lasted for many days. Then I was allowed to keep the baby in prison with me.' When she was relieved of this worry, 'the prison suddenly became an official residence (*praetorium*) for me, where I would rather be than anywhere else.'[71]

Perpetua's first vision was triggered by something her brother said to her: 'You are now in a position of great honour, so great that you should ask for a vision', which would reveal whether it was to be 'suffering or release' ('an passio sit an commeatus').[72] The introduction to the *Passio*, the writer of which is unknown, and elements in the account of Perpetua herself, in her visions and that of Saturus, are unmistakably Montanist rather than orthodox.[73] This is not to say that she and her fellow-martyrs had joined the 'New Prophecy' movement. But it shows that it had reached North Africa from Asia Minor and was influencing her Christian community.[74] The writer declares at the outset that the recent events he is introducing demonstrate the continuing power of the Holy Spirit and that 'new prophecies and new visions' must be honoured as well as those documented in scripture. This is the cardinal point of the differences between Montanus' followers and the church at large.[75] The voluntary martyrdom of Saturus is another strong pointer to Montanist tendencies.[76] Perpetua's first vision was of an ascent by a steep ladder, guarded by a vast and terrifying dragon, which led her up to heaven, helped by Saturus. She came into a vast garden, where a grey-haired man was milking sheep, surrounded by thousands clad in white. The man greeted here and gave her a mouthful of cheese: she took it in cupped hands and chewed it, and 'all who stood around said: "Amen".' To some adherents of the New Prophecy movement consumption of cheese was part of the sacrament, whence they were known derisively as Artotyrites ('bread and cheese people', from *arton*, bread, and *turon*, cheese).[77] In Saturus' vision, the angels said to Bishop Optatus: If you have any disputes between you, settle them among yourselves.' This 'disturbed' Optatus and the priest Aspasius, and the angels added: 'Admonish your congregation, for they come together before you as though returning from the circus and quarrelling about the teams' *factionibus*.[78] The 'factions' in the Christian community in Africa clearly included supporters of the New Prophecy movement, not yet evicted from the

Catholic church. A few years later, some time after 208, one very vocal Christian—Tertullian—was to break with the Catholics, whom he was to denounce as the 'people of appetite', the *psychici*, as opposed to the 'people of the Spirit', *pneumatici*, the Montanists.[79]

Perpetua's first vision was interpreted by her as a sign 'that it would be the *passio* and we began to have no more hope in this world.' A few days later 'a rumour ran about that we were to be given a hearing, and my father arrived from the city [presumably Thuburbo], worn out by weariness, and came up to me' i.e. up the hill, the Byrsa, Carthage's acropolis, where the governor's headquarters and the prison were. Once again he tried in vain to persuade Perpetua to 'give up her pride', to think of her brothers, her mother and aunt, her child. She was sad at her father's distress and that 'he alone from my whole family would not rejoice over my *passio*.' She tried to comfort him: ' "What happens in that dock will be what God wills"…but he left me sorrowfully.' The hearing was not after all to be on that day, but later, with no warning. 'Another day, when we were eating breakfast, we were suddenly hurried away for the hearing and arrived at the forum.' A huge crowd gathered.

> We mounted the dock, the others were questioned and confessed, and it came to my turn as well. And my father suddenly appeared with my son, and pulled me from the steps, saying: 'Worship [the gods], have pity on the infant!' And Hilarianus the procurator, who had then received capital powers (*ius gladii*), replacing the deceased proconsul Minucius Opimianus, said: 'Have mercy on your father's grey hair, have mercy on your infant son: make sacrifice for the emperor's welfare.' And I replied: 'I will not.' Hilarianus said: 'Are you Christian?', and I replied: 'I am Christian.'

Her father renewed his attempt to dissuade her. 'Hilarianus ordered him to be thrown down' and one of the soldiers beat him—an infringement of the normal privilege of the curial class to which Perpetua's father apparently belonged.[80]

'Then he sentenced us all and condemned us to the beasts and we went down to the prison happy.' Perpetua sent the deacon Pomponius to ask for her baby. Her father refused. 'And, as God willed, he did not want the breast any more'; and Perpetua suffered no inflammation of the nipples.[81]

The acting governor Hilarianus, the man from Aphrodisias, did not waste much time over the hearing: no repeated questioning, no threats of torture, no offer of adjournment. One earlier post in his career is now known, and the evidence which supplies it also sheds light on his own religious outlook. Two altars from Asturica (Astorga) in north-west Spain were dedicated by the procurator P. Ael(ius) Hilarianus, together with his children, 'for the welfare of Commodus Augustus, Pious and Fortunate'. Commodus' name was later erased—in the year 193, following the decree of the Senate proposed by Cingius Severus—together with the titles 'Pius Felix', which date the altars to between 185 and 192. The deities worshipped by Hilarianus are instructive. One altar was to Jupiter Best and Greatest, Juno the Queen, and Minerva the Victorious, the Capitoline triad. Hilarianus was a Greek, whose citizenship went back no further than Hadrian's reign, as his names 'Publius Aelius, son of Publius' show.[82] But his native city, Aphrodisias, was one of Rome's oldest allies in Asia Minor, her loyalty proved more than once. Her patron deity Aphrodite was the Roman Venus, divine ancestress

of Rome and of the Julian house.[83] Hilarianus' second altar shows his conservative religious views even more clearly: 'To the Gods and Goddesses to whom human and divine law (*ius fasque*) permit prayer in the pantheon.'[84] Aphrodisias has left no traces of Christianity as early as this. But it lies in northern Caria, divided only by the steep heights of Mount Salbacus from Phrygian cities such as Laodicea on the Lycus, Hierapolis, and Colossae, evangelised by Paul; and not far beyond them in the upper Maeander valley was the Montanist stronghold of Pepuza, the Phrygian New Jerusalem.[85]

Hilarianus' career had not been spectacular, though distinguished enough. At least ten years before 203 he had already reached the 'ducenary' grade in Spain, and the African post was on the same salary level. His son Apollonianus had launched on a centurion's career, which was to take him to the primipilate.[87] His son's name, it may be remarked, reflects the widespread cult of Apollo in Asia Minor. Apollonianus' wife, Tiberia Julia Antonia Letois, is shown by her names to derive from a family enfranchised much earlier, under M. Antonius and Tiberius; and her last name, Letois, recalls the cult of Leto, linked with Apollo as the god's mother, but masking the ancient Anatolian mother-goddess, Cybele.[88] The family is known from two statue bases at Aphrodisias, honouring a grandson named after Hilarianus. They reveal the startling fact that the procurator had ended up as a senator of consular rank. He may well have won special favour from Severus at this time.[89] At all events, the Christians whom he sent to the beasts provided a spectacle to celebrate the birthday of Severus' younger son Geta, on 7 March.[90]

Some time passed between sentence and execution. A few days after the hearing Perpetua had another vision: she dreamed that she saw her little brother Dinocrates, who had died of cancer, standing a long way off, with a great abyss between them, and unable to reach a pool of water to drink. She prayed for him every day 'until we were transferred to the military prison: for we were to fight at the military games on the birthday of Geta Caesar… On the day we stayed in fetters', she had a third dream, this time seeing her brother with his scar healed and able to drink from a golden bowl, and then 'to play happily, as children do'.[91] A few days later, the chief military warder, a deputy centurion ('optio praepositus carceris') named Pudens, began to treat the Christian prisoners with respect, impressed by their inner strength.[92] 'He began to let many visitors in, so that we and they could comfort one another.' Her father came again, wretched with grief, throwing himself on the ground and cursing his old age.[93]

On 6 March ('the day before we were to fight') Perpetua had a fourth vision, the most striking of them all. She dreamed that she was led to the amphitheatre by the deacon Pomponius, to fight not against beasts but in a Greek *pankration*, the brutal all-in wrestling and boxing contest, against an ugly Egyptian. In her dream, after being stripped she suddenly became a man. An impressive figure appeared, splendidly dressed, with a wand and green branch with golden apples, the insignia of the *agonothetes*, the festival president. Perpetua's dream, it is clear, reflected her attendance at, or awareness of, the recently instituted 'Pythicus agon'. She fought and triumphed, and walked towards the Gate of Life ('porta sanavivaria'). On waking she realised that her fight would be with the devil; and that victory would be hers. 'As to what happened at the games, let any who will write of it.'[94]

This concludes Perpetua's account. There follows the vision of Saturus, who saw himself

and Perpetua, already dead, being carried to heaven by four angels. On their way to the heavenly gates they met Jucundus, Saturninus, and Artaxius, 'who had been burned alive in the same persecution, and Quintus who had died as a martyr in prison.' They also met, as mentioned earlier, Optatus, the bishop, and the priest Aspasius, and heard them being admonished by angels. (The Saturninus whom Saturus saw in his dream must have been another man—the name was extremely common—for the Saturninus in the original five died in the amphitheatre with Saturus and Perpetua.)[95]

Perpetua's own account of her time in prison, her dreams, and the dream of Saturus take up well over half of the *Passio*. There follows the unknown author's description of the end.[96] First, the ordeal of the pregnant slave-girl Felicitas is recounted. She was distressed that she might be barred from sharing martyrdom with the others, 'because it is unlawful for pregnant women to be executed and she might have to shed her holy and innocent blood later in the company of others, criminals.' On 5 March her companions prayed 'in united lament' and her labour pains at once started. With much suffering she was delivered of a girl, 'which a certain sister brought up as her own daughter.'[97] The commanding officer of the Carthage garrison, *cohors I urbana,* the military tribune,[98] had drastically tightened up conditions for the prisoners, 'because on the information of very foolish persons he was afraid that the prisoners would be removed from custody by some magic spells.' Perpetua 'spoke to him to his face: "Why do you not allow us to refresh ourselves, the noblest offenders, the Caesar's indeed, since we are to fight on his birthday? Would it not be to your credit if we are brought out on that day looking plumper?" The tribune was shaken and flushed', but ordered that they be treated more humanely, allowing her brothers and others to visit and eat with them—and the author adds, 'the *optio carceris* himself now believed.'[99]

On the evening of 6 March they had their last meal, treating it as an *agape*, the 'love feast' of the early church. A crowd of onlookers had access to them. The Christians addressed them calmly, warning of the judgement of God, proclaiming their own joy in their coming suffering and mocking the crowd's inquisitiveness. Many of those who had come to mock left believing, the author writes. On the morning of 7 March they marched from the prison to the amphitheatre on the edge of Carthage, 'as though they were going to heaven', Perpetua's shining eyes causing the onlookers to look away.[100] At the gate the men were given robes of the priests of Saturn, the women those of Ceres. Perpetua insisted that 'We have come to this point voluntarily on condition that our freedom should not be violated.' The tribune gave in: they were to be brought in as they were.[101] Perpetua began to sing a psalm, Revocatus, Saturninus, and Saturus 'warned the spectators' of the imminent wrath of God. 'When they came within sight of Hilarianus, by gesture and movement of the head they began to tell him: "You [condemn] us, God [condemns] you." The crowd was enraged and demanded that the troupe of gladiators beat them with whips, at which the Christians rejoiced—for the Lord had been beaten thus.'[102]

No doubt because it was Geta's birthday, the beasts were varied. Saturninus and Revocatus were first matched with a leopard, then mauled by a bear after being put in the stocks. Saturus was set against a wild boar, then offered to a bear, which refused to leave its cage. Perpetua and Felicitas were stripped naked, put in nets, and brought out to face a maddened heifer. The

crowd, it is a surprise, was appalled at their nakedness: they were brought back and given tunics. After the first goring, 'the cruelty of the people was appeased, and they were called back to the Gate of Life', where Perpetua was helped by the catachumen Rusticus. She awoke 'as if from sleep—to such an extent had she been in the spirit and in ecstasy', not realising that she had already been in the arena.[103] We may suppose she had been concussed. The Montanist thinking of the writer is apparent in the use of the word 'ecstasy'.[104] Her brother and the other catachumens were also there. She told them: 'Stand fast in faith and love one another, do not be made to stumble by our sufferings.' Saturus at another gate was speaking to the *optio* Pudens. The leopard was let loose on him and he was drenched in his own blood. The crowded roared: 'Well washed! Well washed!', ironic greetings, which the Christians who heard it took as an unwitting allusion to the second washing or baptism, which brought salvation. Saturus asked Pudens for his ring, dipped it in his blood and gave it back. The martyrs were finally despatched by the gladiators' swords, Saturus the first to die, Perpetua guiding the young gladiator's trembling hand to her throat.[105]

Tertullian, writing *Ad Scapulam* ten years later, adds another detail. 'Under the governor Hilarianus, they shouted about our graveyards—"de areis sepultararum nostrarum"—"The yards are ours!" ' The crowd knew the Christians' devotion to their burial places and presumably were demanding that they be confiscated. Tertullian supplies a biting comment: 'The yards were *not* theirs: for they did not have a harvest to thresh', playing on another sense of 'area', that of 'threshing-floor'. In other words, in the summer of 203 the crops failed, the first of the 'recent warnings' which Tertullian addressed to Scapula.[106]

Geta, whose birthday was thus celebrated, was still in Africa, with his father and brother, but far away, in Tripolitania. Indeed Severus himself seems to have been campaigning down in the desert of the Fezzan, to improve the security of his native land against the Saharan tribes.[107] On Severus' own birthday, 11 April, a legionary centurion dedicated an altar at Lepcis for 'the welfare and victory' of the three emperors and the praetorian prefect and 'for their return to their own city'—Lepcis itself.[108] Soon after this the imperial party returned to Rome, and a new proconsul arrived at Carthage, one Rufinus, probably the Apuleius Rufinus who had been consul with Severus in 190. On 27 June Rufinus ordered the execution at Carthage of another Christian woman, the 'virgin Gudden'. 'After four separate bouts of torture on the 'horse' (the horse-shaped rack), and torn by the hooks, long worn out by the filth of the prison, she was finally killed by the sword.' Her name is Punic not Latin. She may have been a survivor of Perpetua's group (perhaps another slave like Felicitas), left behind in prison until the proconsul Rufinus had her despatched.[109]

At about the time of Perpetua's trial, Tertullian wrote the *Scorpiace* to warn his fellow Christians against the temptation of evading persecution.[110] Heretics were abroad, like the scorpion, he proclaimed, Gnostics and Valentinians. Especially when some 'atrocity has crowned the faith of martyrs, they say: "People are perishing for no reason…it is vanity, indeed madness (*dementia*) to die for God… Christ died *once* for us, *once* was killed, so that we should *not* be killed." ' Now 'when the dogstar of persecution burns', 'when Christians are tested by fire, the sword, the beasts, clubs, the hooks, are hunted like hares, are besieged from afar, the heretics attack.' In a striking reversal of his eloquent plea for religious freedom,

Tertullian darkly insists that 'it is not unfitting for heretics to be compelled, rather than enticed, to do their duty.' He compares the Christians' contest with the secular spectacles, including the 'Pythian *agon*' recently instituted at Carthage, of which Perpetua had dreamed. 'Preserve your soul when governors encourage denial!' Christians must suffer, as the apostles suffered.[111]

In the *Scorpiace* Tertullian could speak of Christians being hunted like hares and besieged from afar. A few years later he became a follower of the New Prophecy movement.[112] The Montanists would not permit flight. In his *De fuga* he warned 'brother Fabius' that flight was no solution. He recalls the story of 'the most holy martyr Rutilius', who had so often fled from place to place, but at the last was caught, brought before a governor, tortured, and burned. 'What else did God wish to show us by this case, except that one must not flee, since flight profits nothing if God does not will it?' As for deacons, priests, and bishops who take flight, this is quite wrong. And bribery is no answer either. 'It is wrong for a Christian to "stand firm" with money.'[113]

In the year 211, it seems, an unusual case occurred.[114] At the base of the Carthage garrison the soldiers were paraded to receive a donative from the emperors, presumably Caracalla and Geta, their heads ceremonially adorned with laurel wreaths or crowns to celebrate the occasion. One of the men (Tertullian does not name him) declined to put the wreath on his head, and held it in his hand. Some of his fellow soldiers pointed at him, those further away jeered, those next to him threatened him, soon there was murmuring in the ranks. His name was given to the commanding officer, the tribune, and he was brought before him. 'Why this incorrect dress?', asked the tribune. The man said he was not allowed to do as the others, and, when asked the reason, replied: 'I am Christian.' Judgement followed, the case was registered and he was sent under arrest to 'the prefects'—presumably to the prefects at Rome.[115] The soldier was a *speculator*. 'Now', says Tertullian, 'having taken off his heavy cloak, and sullen *speculator's* boots, and handed over his sword, no laurel wreath even in his hand, he waits in prison for the donative of Christ.' Tertullian had already, in his treatise on *Idolatry*, discussed the possibility of Christians serving in the army, even as a common soldier, who did not have to perform sacrifices or pass capital sentences. 'There is no agreement between the divine and the human oath of engagement (*sacramentum*)...between the camp of light and the camp of darkness... How will he serve in war, indeed how will he be a soldier even in peacetime without a sword—which the Lord has taken away?'[116] Now he devoted a whole work to the subject, first demonstrating at length that the wearing of crowns or wreaths was a form of idolatry, then taking up the central question, 'whether military service is compatible with being Christian.' Ironically the distinction between civilian and military is mentioned, the word for civilian being *paganus* (peasant) which came to mean 'pagan' by contrast with the 'soldiers of Christ'.[117] At the end of the work he makes a curious point: worshippers of Mithras openly declined to wear crowns, but transferred them from their head to their shoulder, saying that the crown belonged to Mithras.

In February 211 the old Emperor Septimius Severus had died, in far-off Britain, where he had been on campaign since 208, with his sons Caracalla and Geta and his empress Julia, and a vast expeditionary force. Caracalla, known officially as Antoninus, had been co-emperor

since 198, Geta, although only a year younger, had remained 'most noble Caesar' until late 209 when he at last became 'Augustus' as well.[118] The situation was tense from the start. Several members of the imperial household were killed, including Caracalla's old 'tutor', Euhodus.[119] The brothers hated each other, and at the end of 211 Caracalla managed to kill Geta, to reign alone. Repercussions clearly followed, not merely the systematic obliteration of Geta's memory. Suspected followers or supporters also suffered.[120] This may have played a part in a new outbreak of persecution, right across the North African provinces, in Mauretania, Numidia, and proconsular Africa. Another possibility is that a momentous decision of Caracalla's in 212, the universal grant of Roman citizenship to all free inhabitants of the empire, followed by mass sacrifice by a grateful population, in which Christians would not participate, sparked off hostility and ensuing trials.[121] At all events, in the *Ad Scapulam*, closely datable by a reference to a recent eclipse of the sun, calculated to have fallen on 14 August 212, Tertullian writes of the 'governor of the legion', i.e. of Numidia, and the governor of Mauretania, harassing Christians with the sword—but not, he stresses, also torturing Christians in an attempt to secure recantation, which is what the proconsul Scapula had been doing.[122]

Scapula is customarily identified with the *consul ordinarius* of 195, P. Julius Scapula Tertullus Priscus. In that case, it is at the least a little surprising that he took up the post as late as 212, 17 years after his consulship. Parallels can indeed be found, but in the early third century an interval of 14 or 15 years was commoner. The answer may be that the proconsul was not the consul of 195, but his cousin, recorded under the names C. Julius Lepidus Tertullus, although it is generally assumed that he was also called Scapula, like his father, uncle, cousins, and grandfather. Lepidus Tertullus had previously served as governor of Numidia, from *c*. 194–7, where he is attested by ten inscriptions, and was consul at the end of his term. Four of these inscriptions show him as a supporter of a pagan cult. At Cuicul he dedicated a temple publicly erected to Tellus Genetrix, Mother Earth; at Verecunda the local council honoured the *Genius patriae Aug.*, and again the legate was the dedicator; at Aquae Flavianae a centurion of his legion erected statues of Aesculapius and Hygia for the welfare and victory of Severus and Albinus Caesar, and Lepidus Tertullus once more officiated. Finally, at the legion's base, Lambaesis, he began a temple to the goddess Caelestis, the old Carthaginian deity Tanit in her Roman guise, completed several years later by one of his successors, Claudius Gallus.[123]

In the early 180s, as is now known, Lepidus Tertullus had begun his career as military tribune of II Adiutrix, at Aquincum (Budapest). There, he dedicated an altar in the Mithraeum of the tribune's house, which makes him one of the earliest Mithraists among the highest echelon of Roman society. The dedication also supplies another detail about this man: he was enrolled in the Roman voting district 'Arnensis'. This makes it almost certain that his home was Carthage[124] and gives added flavour to Tertullian's comments at the end of his letter about the strength of Christianity in that city, where Scapula now resided as proconsul.[125]

Like the *Ad nationes* and *Apologeticum,* the *Ad Scapulam* carries recurrent verbal echoes of Pliny's letter about the Christians and Tacitus' account of the martyrdoms under Nero. Pliny, in his second sentence of his letter, had asked Trajan to 'instruct his ignorance' about the

Christians. Tertullian begins by proclaiming that 'we do not tremble at or fear what we suffer at the hands of the ignorant' and a little later says that 'we are sorry for your ignorance'. Tacitus concludes his brief account of the events of 64 with the comment that the Christians in fact were victims of one man's (Nero's) 'cruelty' (*saevitia*), and this word duly crops up in Tertullian's first paragraph, as do *erumpentes* and *erumpere*, 'burst out', used by Tacitus of the 'deadly superstition'. 'Your gods' (*diis vestris*) 'will not benefit from our sacrifices, if we are forced to this', Tertullian writes—Trajan had laid down that Christians were to be invited to worship 'our gods' (*dis nostris*). Later, Tertullian echoes Pliny's account of how the Christians of Pontus bound one another to eschew crime—theft, adultery, breach of faith, nonpayment of debt. At the end of the letter he triumphantly uses Pliny's language describing the (to him) alarming growth in numbers of the sect: 'all ages, all ranks, both sexes'. Tertullian has 'all sexes [incongruously, a sign of haste], all ages, all statuses' (*dignitatis* rather than *ordinis*—but the word *ordinis* is used a few lines later). Tacitus' 'huge multitude' is also echoed, in a phrase about the large numbers of Christians.[126]

Soon after the opening, the noble plea for freedom of religion recurs, now recast: 'But it is no part of religion to compel religion, which ought to be undertaken freely, not through force.' He reiterates the Christians' innocence of wrongdoing, not least of rebellion, stressing that those found out to be treasonable include persons who had sacrificed for the welfare of emperors and persons who had condemned Christians. Christians are enemies of none, let alone of the Emperor, who is appointed by God.[127] He goes on to warn of divine anger against persecution, first mentioning the failure of the harvest after Hilarianus had shed Christian blood. He recalls the storms of the previous year and the 'fires' seen hanging over the walls of Carthage, the thunderbolts. All these are signs of the imminent wrath of God. What of the eclipse of the sun, he asks, visible in the Utica assize-district? 'You have your astrologers!'[128]

Then he recalls the fate of persecuting governors: Vigellius Saturninus, in Africa, lost his sight. In Cappadocia, Claudius Hieronymianus persecuted Christians through anger at his wife's conversion, and was the only victim of plague in his own headquarters, infested with worms.[129] Referring to the capture of Byzantium from Niger's force in late 195, he recalls how Caecilius Capella, evidently Niger's garrison commander, cried out as he fell into Severan hands: 'Christians rejoice!' He had sent one Mavilus of Hadrumetum to the beasts, presumably as proconsul of Africa in the mid 180s.[130] Scapula should beware of complacency: divine judgement is not always instant. Besides, he urges Scapula, his *mandata* only instruct him to condemn those who confess, not to compel confessors to deny. He then recalls the four governors, Cingius Severus, Vespronius Candidus, Julius Asper, and Valerius Pudens, who refused to sentence Christians to death. 'Your own officials could tell you what benefits they themselves have had from Christians. And how many men of rank have been cured of demons or ill health by Christians! Even Severus himself, the father of Antoninus, was mindful of Christians', he adds. He relates that a Christian called Proculus Tarpacion, in the service of Euhodus, Caracalla's 'tutor', used to treat the old emperor by massage with oil. 'Severus kept him in his palace until the man's death, and Antoninus knew the man to have been a Christian from infancy', he adds.[131] Further, 'Severus not only did not harm women and men of senatorial rank, knowing them to be members of this sect, but actually honoured

them and openly resisted the populace when it was raging against us.' No details are supplied of this remarkable claim.[132] He goes on to attribute to the prayers of Christian soldiers the famous 'Rain Miracle' in Marcus Aurelius' northern wars.[133] Then comes the assertion of innocence of all crime. 'We should not suffer like this—and the governors of Numidia and Mauretania who are now persecuting us use the sword only' (and not torture).[134]

But Christians are not afraid. He tells how Arrius Antoninus, proconsul of Asia a quarter of a century earlier, had declined to execute more than a few of a band of Christians who presented themselves at his tribunal. What will you do, he asks Scapula, if you are faced with 'so many thousands of people, men and women, all sexes, all ages, all ranks, offering themselves to you? How many fires, how many swords will be needed?' Carthage will be decimated, everyone will see relatives and friends there, 'perhaps even men and women of your rank [i.e. senatorial], and relatives and friends of your own friends?' 'But persecution will not cause this sect to diminish—each one who sees such great endurance will be moved to enquire what inspires it, and when he recognises the truth will himself at once follow.'[135]

The Christians who sought voluntary martyrdom at the hands of the Commodan proconsul of Asia, C. Arrius Antoninus (a North African, from Cirta, friend of the orator Fronto),[136] were probably Montanists. Apart from anything else, they derived from Phrygia in the province Asia; and were notorious for seeking voluntary martyrdom. Tertullian, already an adherent when he composed his virulent address to Scapula, would have been well placed to know about this case from his fellow-Montanists. The influence of the New Prophecy movement on the fortunes of Christians from the Antonine period onwards is a subject that deserves further attention; but not in the present paper.[137]

NOTES

1. In spite of the inverted commas, I cannot supply a source for this remark.

2. The title of a famous paper by Ste Croix (1963: 23–33); reprinted in Finley (1974: 210–49) (with Sherwin-White (1964) and Ste Croix (1964) both also reprinted in Finley (1974: 250–55; 256–62). This paper remains the best short treatment of the question, not least on the immensely tricky question of when, why, and in what form the practice of Christianity became a capital offence. The literature on Nero's persecution and on Pliny's encounter with Christians in Pontus is immense. On neither would it seem possible to say anything new, but Baudy (1991), has managed that for the former (although unlikely to find widespread acceptance).

3. It is proper to register my debt to Barnes (1971), and the *Vorarbeiten* (some cited below); it was reprinted, with Addenda (including some candid confessions of error) in 1985. No student of the persecutions or of Roman Africa can neglect this work. Further, on Eusebius' misleading presentation of persecution, as resulting from imperial decision, Barnes (1981) performed an invaluable service.

4. Apart from J.C.M.'s mentor Jones (on whom participants in the Colloquium heard an intriguing paper by Wolf Liebeschuetz), I must single out John Morris, among many other merits founding editor of *Past and Present*, a prosopographer *sans pareil*, rescuer of *PLRE* and an historian of singular charm and humanity; Geoffrey de Ste Croix, my former tutor in Greek history (whose further work on ecclesiastical history is so eagerly awaited); and John Kent, so generous with advice to the numismatically ignorant.

5. *Ad Scapulam* 3. 4–5.

6. Musurillo (1972: 86–9). Musurillo prints the MSS *Praesente bis et Claudiano consulibus* without

comment. There is no doubt about the correct reading, *Condiano*. For these consuls, see e.g. Alföldy (1977: 191 f.). The *cognomen* 'Condianus' seems to be unique to this man and his homonymous uncle, Sex. Quintilius Condianus (*cos.ord.* 151); at least, not even registered by Kajanto (1965). Lane Fox (1986: 751, n. 37) writes that 'a Bruttius also tried the Scillitan martyrs in 180', by mishap confusing the consul with the proconsul (and speculating further that the Bruttius in question could be the historian of that name cited by Eusebius and others, *PIR* ²B 159; Syme (1988: 576 f.) offers thoughts on this writer). The Scillitan martyrs have a considerable bibliography, some items being cited below; cf. also Freudenberger (1973: 196–215).

7. *CIL* III 775 = *ILS* 1116 + add. = *ISM* V 146, Troesmis. That he was legionary legate, *leg. Aug.*, not *pr.pr.*, and that *AE* 1934. 108 = *ISM* V 11, Capidava, has nothing to do with him, was shown clearly by Alföldy (1977: 366 f.). In spite of which he still gets an entry as no. 100, Moesia (inferior), in Thomasson (1984: I, col. 138) (suggesting he was legate of V Mac. and later governor of Moesia inf.). For the transfer of the legion, first to the Parthian war in 162, then, when the war ended in 166, to the reunited III Daciae, Ritterling, *RE* 12.2 (1925) cols. 1578 f.

8. Side: *AE* 1966.470; Perge: unpublished (inspected in March 1990). Saturninus was legate of one emperor, which might (but need not) mean that he was in office before the death of Pius. Lycia-Pamphylia was handed over to proconsuls (in exchange for Pontus-Bithynia) in the 160s, the earliest known being Ti. Julius Frugi, *SEG* 34. 1309. Eck (1986: 526 f., 532) ('Korrekturzusatz', referring to the unpublished Perge inscription), is suitably cautious. That *CIL* XVI 128 of 178 shows a legate in Lycia-Pamphylia might mean simply that the proconsul's legate commanded troops; more likely that the status of Lycia-Pamphylia temporarily changed back to imperial (e.g. after the revolt of Avidius Cassius, who had relatives in the province: his son-in-law was from Patara, *PIR* ²C 859).

9. If Saturninus was indeed legate under Pius (see n. 8) and consul *c.* 162, then he came late to the proconsulship of Africa: for the known intervals, Alföldy (1977: 111 f.). Not impossible, given deaths from plague and war. The latter phenomenon, which led to several adlections, makes it hard to imagine that Saturninus did not govern at least one consular imperial province (even if the evidence for Moesia inferior is delusory, n. 7 above).

10. On this engaging theme see now Salomies (1992: 110), registering our man without offering elucidation. As he shows in this work, attempts to make rules to explain the source of particular sets of extra names (adoption, maternal names, inheritance, etc.) run up against difficulty. But polyonomy does seem to indicate wealth and standing.

11. M. Atilius Metilius Bradua (*cos.ord.*108) and his consular colleague Ap. Annius Trebonius Gallus (from Iguvium) were *consoceri*: their granddaughter Appia Annia Atilia Regilla Caucidia Tertulla was the wife of the great Herodes. Details on most of these people in *PIR* ² A and C, the latest refinements in Raepsaet-Charlier (1987: nos 66, 118, 202, etc.). 'Caucidius' was 'preternaturally rare', Syme (1988: iv, 293); otherwise only borne by L. Mummius Niger Q. Valerius Vegetus Severinus Caucidius Tertullus (*CIL* IX 948, Aecae), a senator with some Spanish connections (elucidated by Syme, loc. cit.).

12. Rhodes: thus Stiglitz, *RE* 8A.2 (1958) col. 2570, but only because of the Rhodian *demotikon* 'Plarios'. Mere coincidence, surely (but the notion was incautiously sponsored by Alföldy, (1977: 320)). For senatorial Plariae, see Raepsaet-Charlier (1987: nos 101, 612). 'Raius' is not very common, except in Apulia, e.g. C. Raius Capito, a *praefectus fabrum* (*CIL* IX 376, Larinum); there was one at Italica though (*CIL* II 1129), 'Vigellii' crop up in various parts of Italy, particularly Campania; a P. Vigellius at Cures Sabini is the only Publius (*CIL* IX 4998), apart from *liberti* in Calabria (IX 255.174). There is a strong temptation to produce ancestors by emendation: a 'Vitellius Saturninus', prefect of a legion in 69 is registered by Tacitus, *Hist.* 1.82.1; and the *Acta Fratrum Arvalium* show a Hadrianic Arval Brother called 'P. Vitellius Satur[ni]nus'. Scribal error in Tacitus is easily acceptable (given the number of times 'Vitellius' occurs in the *Hist.*); and mason's errors occur in the *Acta Arv.*, e.g. 'Octavianus' for 'Optatianus' (*CIL* VI 2086, cf. *PIR* ²F 38). But two emendations are no doubt excessive and in any case

the legionary prefect and the Arval Brother, even if ideal as grandfather and father of our man, do not tell us any more about his *origo*.

13. For details, Birley (1987).

14. For discussion (or debate), see Frend (1965: 361 f.); Barnes (1971: 60 ff., 361 ff.). To which I would add only the conjecture that Christianity could well have spread through transfers among the *familia Caesaris*, of which a substantial contingent was always present in Africa, especially at Carthage.

15. Barnes (1971: 63) discusses the evidence. I suppose that 'vicus Scillitanorum' at Carthage, where the martyr Felix was interred (Musurillo 1972: 270–1, c. 31 of the *Passio S. Felicis Episcopi*) may have got its name from the martyrs' place of execution or burial (n. 6). On the other hand, if the name was already used under Diocletian, it would probably simply be an existing name, suggesting either that Scilli was on the outskirts of Carthage or that it was not far away.

16. Karpp (1961: 165–72) argues that there were originally only six names in the text, the second six being inserted later. There seems no solid reason to accept this; no doubt what we have is only an abbreviated version of the proceedings in any case.

17. Frend (1965: 313), writes that 'from their names, [they] seem...to have been drawn from the native, non-citizen element of the population', hardly a tenable inference. He also attributes to Vigellius the despatch of another group, whose names certainly are 'native', Namphamo, Miggin, Lucitas and Sanae, martyrs of Madauros. But Barnes (1971: 261 f.), gives good grounds for the view that Namphamo and company were Donatists, fourth-century victims of Catholic persecution. (His argument about the time of year that proconsuls began their term lapses, however, as he concedes later (1985: 333): the proconsular year in fact ran from April to April, there seems no doubt.) On the names, cf. Kajanto (1965) 297 (Speratus), 298 (Donatus), 272 (Felix), 218 (Januarius), 292 (Secunda), 330 (Aquilinus), 313 (Generosa). The evidently 'native' Cittinus and Nartzalus are discussed by Barnes (1971: 63 and n. 9). Vestia and Laetantius seem unattested and one might think of emending, the former example to the common 'Vettia', the latter to 'Lactantius'. But this is hardly necessary. Kajanto (1965: 101) notes that 'the formatives -*ius/ia* and -*osus/sa* were largely Christian'. (ibid.: 115 ff., 122 f.). Veturius is common enough: over thirty specimens in *ILS* alone. That this person alone among the twelve was referred to by *gentilicium* is not a difficulty (one could of course take it as a *cognomen*, or emend to Veturinus), see the Fabius and Rutilius in Tertullian's *De fuga* (below).

18. Mommsen (1899: 918 ff.); and Crook (1967: 271 ff.).

19. Eusebius *Hist. Eccl.* 5.1, esp. l6 ff.

20. Pliny, *Ep.* 10.96.4 'fuerunt alii similis amentiae, quos, quia cives Romani erant, adnotavi in urbem remittendos' (Roman citizens were no longer sent to Rome for trial by now, see Eusebius, *Hist. Eccl.* 5.1.44: the governor of Lugdunensis, a few years before 180, learning that Attalus 'was a Roman, commanded him to be put back in jail with the rest, about whom he had written to the emperor and was awaiting his reply.' In 5.1.47 M. Aurelius' reply is duly mentioned: 'Caesar had written that they should be tortured to death, but if any should recant they should be released'). MENTI LAVDANDAE, a unique coin legend, appears a few years later under Pertinax, *RIC* IV.1 Pertinax no. 7, see p. 4. On the goddess (Bona) Mens, Marbach, *RE* 15.1 (1931) col. 936 f. (worshipped from 215 BC onwards). Pertinax, born in 126 (*HA Pert.* 15.6), was more or less an exact contemporary of Vigellius and no doubt shared the same traditional values. The coin legend, of course, expresses a return to sanity after Commodus' excesses (see below, n. 52 and n. 21).

21. Barnes (1971: 277) c.6, notes that 'illi Deo servio, quem nemo hominum vidit nec videre his oculis potest' is a quotation from Paul, *I Tim.* 6.16, but in a different text—with *his oculis* added, and other variations—from other early Latin versions. See also n. 24 below. I owe the translation here given of 'mysterium simplicitatis' in c. 4 to Den Boeft and Bremmer (1981: 45 ff.). They also offer illuminating comment on 'dementia' and 'bona mens', the latter being urged on Cyprian by the proconsul Galerius Maximus in 258, *Acta Proconsularia* 4.1, as in Lanata (1973). The sentence in question is not

registered by Musurillo (1972: 172), even in the apparatus.

22. E.g. Tatian, *Oratio ad Graecos*. Tertullian was to excel in this genre, in particular in Book 2 of the *Ad nationes* and in the *Apologeticum*, not to mention the *De spectaculis*, *De idololatria*, *De corona militis*.

23. Den Boeft and Bremmer (1981: 47 f.), convincingly explain that Vigellius' use of *numquid* here is a little more complex than 'a question expecting the answer No': it is a question which...'is the equivalent of an appeal', which they paraphrase as 'I am afraid you will disregard my advice, but would it not be wise to take some time for reflection?'

24. *Acts* 13.13–14; 14.25.

25. Discussed by Barnes (1971: 276 ff.). König (1990: 363–73), suggests that the Scillitani were Marcionites on the basis of this stress on Paul. Marcion drastically cut down the scriptures, retaining only one gospel (Luke, edited) and ten Pauline epistles, not including, however, the Pastoral epistles *Tim.* and *Tit.*, which he either rejected or perhaps did not know. For a summary with bibliography, see *ODDC* [2] (870 f.). That Speratus quoted precisely *I Tim.* (n. 21 above) is a difficulty that König attempts to circumvent, not wholly convincingly. There is no reason why the Marcionites may not have been represented in Africa in 180—not so many years later Tertullian was to devote a vast work to rebutting the heresy. But the sentence about Paul is hardly evidence enough. For use of Greek by African Christians, see below and n. 64. Still, one cannot question that a Latin translation of the scriptures was already in existence, see Barnes (1971: 276 ff.).

26. *Pass. Scill.* 14 ff.; cf. n. 18 above.

27. The full names only in later codices, cf. *PIR*[1] (S 324). For what it is worth, 'Florens' is rather rare, only six epigraphic examples, plus two Christian ones, being known to Kajanto (1965: 233) (cf. ibid. 116 on present participle *cognomina*), so too 'Tertullianus' (ibid. 199), two senators and twelve others. Cf. also Birley (1988b: 9), for the conjecture that the *gentilicium* 'Septimius' was favoured in Africa by newly enfranchised persons because of its resemblance to the word (or name) *špṭ*, pl. *špṭm*, Punic 'sufes'. Barnes (1971: 3 ff.) argues forcefully that the evidence on Tertullian's person and parentage supplied by Jerome, *De viris illustribus* 53, has no independent value; in particular, that the statement that his father was a 'centurio proconsularis' derived from Jerome having the same (corrupt) reading of *Apol.* 9.2 as the Fulda version. The passage refers to the suppression of human sacrifice in Africa: 'infantes penes Africam Saturno immolabantur palam usque ad proconsulatum Tiberii...teste militia patris nostri, quae id ipsum munus illi proconsuli functa est.' The better reading, Barnes argues, should be 'patriae nostrae'. This must remain uncertain. But the argument that it is inconceivable that human sacrifice was only finally suppressed *c.* 150, restated later (1985: 324 f.), is also open to question. Tertullian's point would be even more telling if he was asserting that such barbarity had continued within recent memory. Clearly 'Tiberii' is corrupt. John Morris' conjecture 'Hiberi' (cited by Thomasson [1969: 178]) is dismissed by Barnes (1985: 325) as being without merit. It is in fact palaeographically brilliant: cursive H is so often written with the upright not properly joined to the remainder of the letter. Once that was the case, it would be natural to read 'tiiberi', and to correct this to 'Tiberii'. M. Antonius Hiberus (*cos.ord.* 133; *PIR* [2] A 837, cf. 836) could easily be accommodated in the *Fasti* of Africa *c.* 148–150; Thomasson (1984: col. 390 no. 164) continues to note the conjecture. It is therefore still legitimate to think of T. as the son of a centurion, whether of III Augusta, with the cohort on secondment to the proconsul, or of the *cohors urbana*. It is another matter to identify the Christian Tertullian with the jurist of that name, cf. against this Kunkel (1952: 236 ff.); Barnes (1971: 22 ff.).

28. Barnes (1971: 32 ff.).

29: Ibid. 38 and cf. n. 122.

30. Barnes (1985: 328).

31. *HA Cl. Alb.*1.3, 4.1, cf. *Sev.* 11.3, regarded as fiction by some, but defended by Birley (1981: 147), with appeal to the coinage of Albinus with Saeculum Frugiferum. For his African supporters, Alföldy (1968: esp. 120).

32. Pflaum (1960–1) nos 222, 228, 239, 240 (only the first specifying Africa).

33. *Ad mart.* 6.2, cf. Barnes (1971: 32 f.).

34. *Ad nat.*1.17.4, cf. *HA Sev.* 11.9–12.1.

35. *Apol.* 5.4–6.

36. *HA Sev.* 15.4–5.

37. *Ad mart.* 1.1; 4.2–3.

38. He names Pliny and refers directly to *Ep.* 10.96–7 in *Apol.* 2.6. To argue the thesis that the *Ad nat.* and *Apol.* (and to an extent the *Ad Scap.* which repeats material from the earlier work briefly) represent to a large extent a reaction, indeed an angry rebuttal, of Tacitus, *Ann.* 15.44 and Pliny, *Ep.* 10.96–97, requires more space than I could expect the editor of this journal to allow me. It requires the citation of hundreds of passages from the relevant writings of Tertullian. For a few indications, see n. 126 below.

39. *Ad nat.* 1.2.1–2.

40. *Apol.* 1.1.

41. Cf. above all Ste Croix (1963, 1964).

42. *Apol.* 2.5, cf. Eusebius, *Hist. Eccl.* 5.1. 14, 26; Fronto, ap. Minucius Felix, *Octavius* 9.8. Benko (1984: 54 ff.), gives a full discussion. Pliny, *Ep.* 10.96.2 refers to the 'flagitia cohaerentia nomini' and admits that he could not detect them, 'nihil aliud inveni quam superstitionem pravam et immodicam,' 96.8, and Trajan ruled 'conquirendi non sunt,' 10.97.2, *Apol.* 2.6 ff. brings out the illogicality of the Roman position.

43. *Apol.* 2.10 ff.; 2.14; 9.6; 12.3–5.

44. *Apol.* 26.6; 27.1–2 (cf. Pliny, *Ep.*10:96.3); 30.4 (an interesting set of priorities); 30.7; 30.7.

45. *Apol.* 35.8–12; 37.1–2; 40.1–2 (perhaps the most famous sentence in the work).

46. *Apol.* 44.1–45.1; 45.7; 50.1–3.

47. *Apol.* 50; 12–13; 50.16.

48. The point needs stressing because of the mistaken view that there was a 'Severan persecution' launched in 202, properly rebutted by Schwarte (1963: 185–208). If anything, Severus was rather favourable to Christians, cf. below and n. 136. At all events, *HA Sev.* 17.1 ('Iudaeos fieri sub gravi poena vetuit. idem etiam de Christianis sanxit') should not be treated as factual evidence. The truth is that persecution depended on the response of the man on the spot, not—until Decius—on an imperial edict.

49. I have treated Scapula and one of the three persecutors whose fate is cited, Caecilius Capella, as well as the four lenient governors, in a recent article: Birley (1991: 81–98), to which I shall refer from time to time in the ensuing notes, although not refraining from quoting evidence directly.

50. *Ad Scap.* 4.1–4.

51. *CIL* VIII 2752; *AE* 1955.136; 1967.575, all Lambaesis, show him legate of III Augusta under Marcus, probably between 174 and 176; the first gives his *origo*, the third his last two names, the second and third show him as consul (or *co[s. desig.]*). Dacia: *CIL* III 1092, Apulum. Dio 78.17. *HA Did. Jul.* 5.6. Note that Tertullian calls the four lenient governors 'praesides' (4.3); a little later he can refer to the legate of III Augusta (or Numidia) as the 'praeses legionis' (4.12). As a matter of fact, it is hard to find a space for Candidus as proconsul of Africa in *c.* 190, Thomasson (1984: cols. 384 f.); Leunissen (1989: 213 f.), although both squeeze him in.

52. Leunissen (1989: 215); Birley (1991: 88 f.), the former proposing, plausibly, 196–7 for L. Cingius' proconsulship. The man in office in 198–9 was L. Cossonius Eggius Marullus (*cos.ord.* 184; *PIR* ² E 10). No name is yet available for 197–8, when Tertullian, writing *Ad mart., Ad nat.* and *Apol.*, makes it clear that persecution was in swing. Cingius in the Senate: *HA Comm.* 20. 1–4. The months: Dio 72.15.3 f.; *HA Comm.* 11.8, etc.; *divus Commodus*: e.g. *CIL* VIII 9317; Dio 75.7.4, etc. The purge: Dio 75.8.4; *HA Sev* 13.1–7 (a list of 42 names, some invented); 13.9 (Cingius).

53. *PIR* ² (J 182) supplies necessary documentation. For Paul at Antioch, *Acts* 13.14 ff .; Attaleia, 14.25.

54. Pudens: Birley (1981: 155 ff.); Thomasson (1984: col. 386). Vazitana: *CIL* VIII 11999 = *ILS* 5441. Fundanus: the rescript is known now only from Eusebius, *Hist. Eccl.* 4.9.1–3, although it was originally, in Latin, quoted by Justin, *Apol.* 1.68, where the text now has Eusebius' Greek version. I admit to sharing the scepticism of Nesselhauf (1976: 348–61), about this document, although prepared to believe that Hadrian did write to Fundanus about Christians.

55. For details on the visit, see Birley (1988a: 146 ff.).

56. On the *agon* Barnes (1969: 125 ff.); Robert (1982), which need not undermine Barnes' argument that Severus granted the *agon* during his visit. What is impressive in Robert's paper is the demonstration that the dream of Perpetua (below) is full of imagery of the 'Pythicus agon'. Tertullian, *Scorp.* 6.2, refers to the grant. Cf. also Robert (1978: 468 f.). and Barnes (1985: 328 f.).

57. Ward-Perkins (1948: 59 ff.); (1951: 268 ff.).

58. *CIG* 2792–3, Aphrodisias, supplies the *origo*, cf. further, nn. 82 ff. below.

59. *Pass. Perp.* 6.3: 'et Hilarianus procurator, qui tunc loco proconsulis Minuci Timiniani' (or Oppiani, variant reading) 'defuncti ius gladii acceperat.' The name of the deceased proconsul is now established as Minucius Opimianus, as conjectured by Birley (1971: 221 n. l), cf. *PIR* ² M 622.

60. The date of the martyrdom, 7 March 203, is the key. On this see Barnes, *JTS* 19 (1968: 522 ff.). There is a vast bibliography to this *Passio*, described by Delehaye as *le chef-d' oeuvre de la littérature hagiographique* (1921: 49). Only a few items can be referred to in the notes that follow. Musurillo (1972: 106 ff.) gives the main Latin recension, with translation.

61. *Pass. Perp.* ²

62. Delehaye (1921: 53), simply dismissed the information about Thuburbo Minus in a sentence (partly because the date is wrongly given in the other versions as under Valerian), followed by *inter al.* Barnes (1971: 72). I find Thuburbo Minus perfectly plausible. For the other *acta,* Van Beek (1936), esp. the Greek version 2.1.

63. 'Honeste nata': cf. the still classic study of Hirschfeld, Die Rangtitel der römischen Kaiserzeit (1913: 646–81 esp. 679 ff.). 'honestus' indicated equestrian rank or the 'höheren Munizipalkreisen' and epigraphic use came in at latest under Caracalla.

64. Robert (1982: 228 ff.), demonstrates that *Pass. Perp.* l0. 5–15 should be read in the Greek version because Perpetua wrote (or dictated) in Greek. Dinocrates: *Pass. Perp.* 7.1 ff. Fridh (1968: 58 f.) argues persuasively that Saturus' first language was Greek. On the use of Greek at Carthage, Barnes (1971: 67 ff.). Perpetua's *gentilicium* Vibia probably indicates descent from someone enfranchised during the proconsulship of A. Vibius Habitus or C. Vibius Marsus (the latter in office for three years) under Tiberius: Thomasson (1984: cols 373 f.).

65. *Pass. Perp.* 2.2, 3.7, 4.1, 20.10.

66. *Pass. Perp.* 2.2, 3.2 ff., 5.1 ff., 6.1 ff., 9.2 f. (father); 2.2, 3.8, 5.3 (mother); 5.3 (aunt).

67. *Pass. Perp.* 4.5.

68. *Pass. Perp.* 3.7 (Tertius and Pomponius); 11.8 (Jucundus, Saturninus, and Artaxius, 'burned alive in the same persecution', Quintus); 13.1 (Optatus and Aspasius); 14.2 (Secundulus). Den Boeft and Bremmer (1982: 392) note, following Fridh (1968: 70 f.) (n. 64) that the Greek version calls 'the other Saturninus' (11.8) 'Saturus' and that this should probably be accepted. 'Saturninus' was immensely popular in Africa, it is true, Kajanto (1965: 213) (1163 out of a total of 2507); but as they comment, the Greek form 'Saturus' was also popular in Africa. Note that 'Aspasius' and 'Artaxius' are also Greek.

69. For clergy ministering to Christians in prison, cf. *Pass. Montani et Lucii*, Musurillo (1972: 214 ff.) 9.2 (a sub-deacon—also two laymen), 259. On this whole question (and much else), see Wipszycka (1987).

70. *Pass. Perp.* 3.1; for 'prosecutores' cf. *Digest* 48.3.7 'solent praesides provinciarum, in quibus delictum est, scribere ad collegas suos, ubi factores agere dicuntur, et desiderare, ut cum prosecutoribus ad se remittantur.'

71. *Pass. Perp.* 3.1–8.

72. *Pass. Perp.* 4.1.

73. It has often been argued that Tertullian was the 'editor' of the *Passio*; but for arguments against see Barnes (1971: 79 f.; 1985: 329). For one thing, in the *De anima* 55.4 he attributes to a vision of Perpetua an item that occurs in that of Saturus. On the Montanism of the *Passio*, Barnes (1971: 77 ff.); later modified a little (1985: 329).

74. On Montanism the two works by Labriolle remain fundamental (1913a; 1913b). For a brief defence of the earlier date of inception (c. 156, rather than c. 170) see Birley (1987: 259 f.).

75. Stated clearly in the (hostile) account by Eusebius, *Hist. Eccl.* 5.16–18.

76. Saturus: *Pass. Perp.* 4.5 ('qui postea se propter nos ultro tradiderat'). On 'voluntary martyrdom', note the perceptive remarks by Ste Croix (1963: 21 ff.), playing down the view that it was confined to 'heretical or schismatic sects such as Montanists and Donatists'; but conceding that 'the positive evidence…begins…about 150…it could have been a Montanist practice in origin', although going on to argue that it began before Montanism.

77. *Pass. Perp.* 4.3–10 ('caseo', c. 9, mistranslated as 'milk' by Musurillo 113). Epiphanius, *Panarion* 49.5 (Labriolle, 1913b: 141); Philastrius, *Liber de haer.* 74 (ibid.: 151), refer to the Artotyrites. Amat (1989: 182) doubts the cheese as evidence for Montanism.

78. Saturus' vision: *Pass. Perp.* 11.2–13.8.

79. The chronology of Tertullian's adherence to Montanism is difficult to establish. Barnes makes a valiant attempt at dating the whole corpus (1971: 30 ff.), with valuable list of works with Montanist language (ibid.: 43 f.); but later retracts to some extent (1985: 325 ff.); in particular concluding, that 'a date later than 208' is necessary for all the Montanist works.

80. *Pass. Perp.* 4. 10–6.5. 'curial class', cf. nn. 18, 63 above.

81. *Pass. Perp.* 6.6–7.

82. *AE* 1968.227–8, datable between 185 and 192 by Commodus' erased but still legible titulature (*Pius Felix*).

83. *CIG* 2792–3. On the city and its relations with Rome, see Reynolds (1982).

84. *AE* 1968.228, the significance of which was stressed by Barnes (1971: 163).

85. The search for 'the New Jerusalem' has so far proved unavailing. For an engaging account of a search, see Strobel (1980). But the general area, the upper Maeander valley, is beyond dispute.

86. Pflaum, (1960–1: 1092), does not venture a salary grading for Hilarianus; but the fact that he replaced the proconsul must indicate that he was a senior procurator. In the supplement he listed P. Aelius Hilarianus under the procurators of 'Hisp.cit. per Asturiam et Callaeciam', but unfortunately did not assign him a separate *carrière*—perhaps regarding the identity with the persecutor as uncertain (1982: 117).

87. *CIG* 2792–3, as interpreted by Dobson (1978: 296 f.).

88. Cf. e.g. Wehrli, *RE* Supp. 5 (1931), art. 'Leto', cols 555–76, esp: 573.

89. Cf. n. 87 above.

90. *Pass. Perp.* 7.4, cf. n. 60 above.

91. *Pass. Perp.* 7.1–8.4.

92. *Pass. Perp.* 9.1. The only other attested prison warder is precisely a man in *coh. I urbana*, *CIL* IX 1617 = *ILS* 2117 + add., discussed by Domaszewski (1967: VIII, 18 f.); at length by Freis (1967: 44, 51, 71–5). 'Optio carcaris' (sic) was only one post in a long career that ended in 134, probably before *coh.I* was switched to Carthage (ibid.: 34). Freis does not discuss Pudens (cf. ibid.: 35), presumably because he thinks the detached cohort of III Aug. was involved, a view also standard in the case of *De corona militis* (below, n. 114).

93. *Pass. Perp.* 9.2.

94. *Pass. Perp.* 10, brilliantly elucidated by Robert (1982: 228 ff.).

95. *Pass. Perp.* 11–13. (Cf. n. 68 above on 'the other Saturninus').

96. *Pass. Perp.* 14–21. On the authorship question, n. 73 above.

97. *Pass. Perp.* 15.

98. *Pass. Perp.* 16.2 ff., 18.6, mentions the tribune, not named. No other tribune under Severus is known; L. Julius Victor, *AE* 1949.108, a man from the province Africa, was centurion in the *coh. I urb.* at this time, promoted from *laterculensis* of the Guard Prefects.

99. *Pass. Perp.* 16.2–4.

100. *Pass. Perp.* 17.1–18.2.

101. *Pass. Perp.* 18.4–6. Saturn was, of course, the Roman version of the Carthaginian Ba'al, and as such the most honoured deity in the province, cf. Leglay (1966). Ceres is perhaps more likely to be the Carthaginian Tanit pene-ba'al, though the latter was usually labelled Caelestis (Juno); cf. Freudenberger (1973–4: 180); the cult of the Cereres (Demeter and Persephone) had been brought to Carthage from Sicily in 396 BC, Diodorus 14.77.4 f. Whether the dressing up of prisoners was a ritual substitute for the banned human sacrifice (Tertullian, *Apol.* 9, n. 27 above) or simply a 'fatal charade' to make the occasion more entertaining, is not clear. Coleman (1990: 66), regards either as possible. In the context of the fascinating material she discusses for the latter (ibid.: 44–73), simple gruesome showmanship seems rather plausible—but these shows were fundamentally religious, as Tertullian, *De spectaculis*, stressed. Coleman does not refer to Freudenberger (1973–4), who discusses these questions in relation to the *Pass. Perp.* Her comment that 'it was apparently customary to force prisoners at Carthage (or maybe only Christian prisoners) to wear these robes' seems fully justified.

102. *Pass. Perp.* 18.7–9.

103. *Pass. Perp.* 19.1–20.8.

104. *Pass. Perp.* 20. 8; 'adeo in spiritu et in extasi fuerat'; cf. 21.11.

105. *Pass. Perp.* 20. 8–21, 10.

106. Tertullian, *Ad Scap.* 3.1.

107. Birley (1988a: 151 ff.).

108. *IRT* 292, as interpreted convincingly by Guey (1950: 55 ff.).

109. *Martyriologium Adonis* (*PL* 133.304); Augustine, *Serm.* 294; discussed by Barnes (1971: 266 f.); slightly emended later (1985: 334). If, as he argues, Rufinus was the same as Minucius Opimianus (quite possible, cf. now *PIR* ²M 621–23a for the names T. Salvius Rufinus in the nomenclature of Opimianus' grandfather and great-grandfather), the martyrdom would have fallen in the previous year, 202. A proconsul Q. Caecil[ius...] is now known for 202, assigned to 201–2 by Thomasson III (1990: col, 44 n. 116a). He *might* be a Q. Caecilius Rufinus, although there are problems in dating persons of this name, Thomasson (1984: cols 113 n. 17, 119 n. 51, 367 n. 56). For Apuleius Rufinus Leunissen (1989: 132, 208 f., 216) (not however aware of Q. Caecil[ius]...; cf. 201, etc. on Q. Caecilius Rufinus—if the legate of Pannonia inferior of this name were, after all, Severan, he would not in any case have been consul soon enough to be proconsul of Africa in 202; *non liquet*).

110. The date of the *Scorpiace* is of course uncertain, Barnes (1969: 125 ff.; 1971: 34 f.), puts it late 203 or 204; quite properly defended later (1985: 328 f.). Den Boeft and Bremmer (1982: 391) meanwhile argue with some plausibility that it could belong to the interval between the first celebration of the 'Pythicus agon' (*Scorp.* 6.2, implying a recent institution of the event) and the death of Perpetua and her fellows: 'For, if he had composed *Scorpiace* shortly after their death, would he not have alluded to that event?'

111. *Scorp.* 1.5 ff.; 1.10–12; 2.1; 6.2; 11.3.

112. On the problems of dating his move to Montanism, n. 79 above.

113. *De fuga in persecutione* 1.1; 5.5; 10.1; 10.2; 11.1; 11.4; 12.1; 13.6. Note that his Montanism is established (apart from the entire argument against flight) by 1:1, 14.3, references to the '*paraclitus*'.

114. The case described in *De corona militis* is dated confidently to 211 by Freudenberger (1970:

579–92); Barnes (1985: 328) remains a little hesitant. Freudenberger (loc. cit.: 587 ff.), argues that the unit involved was the detached cohort of III Aug., not the *coh. I urbana*, because the Christian soldier was a *speculator*. He also thinks men of III Aug. were responsible for imprisoning Perpetua and companions—but cf. n. 92 above on the *optio carceris*, which points the other way for the case in 203. Even if the proconsul had six *speculatores* of III Aug. seconded from Lambaesis, as inferred from *CIL* VIII 2586 by Domaszewski (1967: 32), he could have had others who served in the urban cohort. We simply do not know enough about the *principales* of the *cohortes urbanae*; nor about the *officium* of the proconsul. *Prima facie*, the soldier's despatch 'to the prefects' (next note) ought to mean not only that the episode took place at Carthage (not Lambaesis), but that the ultimate commanding officer was the man under whom the *coh. urb.* came. I see no reason to accept the argument that legionary *speculatores* were automatically seconded from the Guard. Whether the ultimate commander of the Carthage *coh. urb.* was the Guard Prefect(s) or City Prefect is a further problem; that the City Prefect was responsible under Hadrian, as shown by *CIL* IX 1617, does not prove what the position was under Severus and Caracalla.

115. *De corona militis* 1:1–3, on which cf. Franchi de'Cavalieri (1935: 357–6, 361 f.).

116. *De idololatria* 19.1–3. The whole question of Christians and military service before Constantine is discussed again by Helgeland (1979).

117. *De corona militis* 11.5: 'apud hunc [sc. Christum] tam miles est paganus fidelis, quam paganus est miles fidelis.' The whole of c. 11 is fascinating for insight into military life. Quite what is meant by the *miles/paganus* antithesis is far from easy to follow: 'with him a Christian civilian is just as much a soldier (i.e. 'miles Christi') as a Christian soldier is a civilian' (i.e. should not kill, etc.) is how I understand it. Mohrmann (1952: 109–19), shows that 'paganus' did not yet mean 'pagan'. (I cannot follow her interpretation of *De corona militis, 11.5*, in other respects.)

118. Birley (1988: 170 ff.). For the elevation of Geta, however, I am persuaded by Margaret Roxan that the date of 209 should be retained. It is worth registering here Morris (1968) with arguments that require to be taken seriously for attributing the martyrdom of Alban to Geta, left in charge in the south while Caracalla and Severus were fighting the Caledonians.

119. Dio 77. 1.1, cf. below and n. 134.

120. Birley (1988: 189). Barnes (1968: 51 f.), shows that Geta was killed on 26 December 211 (not February 212).

121. Thus e.g. Frend (1965: 312), based on the reference to general sacrifice in *P.Giss.* l. (One will not necessarily accept his remarks about Sarapis.) That the *constitutio Antoniniana* has been dated by some to 214 not 212, and that *P. Giss.* 1 is believed by some not to refer to it anyway, are matters which would require too much space to discuss here.

122. *Ad Scap.* 3.3, the almost complete eclipse visible 'in conventu Uticensi', can be dated to 14 Aug. 212, Barnes (1971: 38). See 4.12 on the governors of Numidia and Mauretania, 4.2 on Scapula's torturing.

123. For detailed discussion, Birley (1991: 81–2, n. l).

124. Published at last by Kocsis (1989: 87 f.) no. 4; for the significance of 'Arn', cf. previous note.

125. *Ad Scap.* 5.2, cf. below and n. 134.

126. Cf. remarks in n. 38, above. Pliny, *Ep.* 10.96.1 'quis enim potest melius vel cunctationem meam regere vel *ignorantiam* instruere?'; cf, *Ad Scap.* 1.1, 1.4 and very frequently in *Ad nat.* and *Apol.* Tacitus, *Ann.* 15.44.5: 'in *saevitiam* unius absumerentur'; *Ad Scap.* 1.2 (and in the earlier works). 44.3: 'exitiabilis superstitio *erumpebat*'; *Ad Scap.* 1.2,4; 5.1; and in earlier works. Pliny, *Ep.* 10.97.2: id est supplicando *dis nostris*, *Ad Scap.* 2.2: 'ita etsi nos compuleritis ad sacrificandum, nihil praestabitis *diis vestris*—di vestri' are mocked throughout *Ad nat.* and *Apol.* Pliny, *Ep.* 10.96. 7, echoed in *Ad Scap.* 4.9 (and explicitly referred to Pliny in *Apol.* 2.6). 10.96.9; 'omnis aetatis', etc; *Ad Scap.* 5.2, Tacitus, *Ann.* 15.44.4: 'multitudo ingens'; *Ad Scap.* 2.10 'tanta hominum multitudo'. A host of further parallels or echoes could be supplied from *Ad nat.*, *Apol.* and other works of Tertullian. Above all, Tacitus' *odio* ('humani generis') (15.44.4) recurs constantly from *Ad nat.* 1.1 onwards, and to explain it as simply a

topos, 'ignorantia/odium', as commentators have, is manifestly inadequate. But fuller treatment must be reserved for a later occasion.

127. *Ad Scap.* 2.2 ff.

128. *Ad Scap.* 2.11–3.3.

129. On Saturninus, above, at length. Hieronymianus: previously legate of VI Victrix; he erected a temple of Serapis at York, *RIB* 658. On this man, Birley (1981: 263 ff.). The MSS call him 'Claudius Lucius Hieronymianus' (with some variant spellings of the latter name). I now incline to think he was really called 'Claudius Julius Hieronymianus' (rather than 'L. Claudius') 'Lucius' for 'Julius' is an easy textual corruption; 'Claudii Julii' can be found in the east, cf. Holtheide (1983: 275) nos 11–12; 298 no. 174. 'Lupus' is also a possible emendation. But perhaps 'Lucius' was just used as a *cognomen*, Kajanto (1965: 172).

130. *Ad Scap.* 3.5–6. I have argued the case for retaining the MSS reading which makes Capella a persecutor in Africa (with stress on an overlooked inscription, *AE* 1972.658, which shows he was governor of Cilicia) in Birley (1991). Majority opinion has emended otherwise (the text cannot make sense without some kind of change), and thus attributes the sentencing of Mavilus to Scapula.

131. *Ad Scap.* 4.6, reading 'Euhodi procuratorem....quem et Antoninus noverat lacte Christiano educatum'. The reading 'educatus'—giving Caracalla a Christian wetnurse—is supported by several MSS, but must probably be rejected (a pity, but there it is). Euhodus had been killed by Caracalla just before this (Tertullian may not have known), n. 119 above.

132. *Ad Scap.* 4.7, much discussed, recently by Durst (1988) 5.2, 'tui ordinis viros et matronas', is likely to emerge as victims of persecution at Carthage if Scapula does not draw back, is also relevant. At the very least, these remarks make the notion of Severus initiating a persecution by edict hard to swallow (n. 48 above).

133. *Ad Scap.* 4.7, much discussed also.

134. *Ad Scap.* 4.9–12.

135. *Ad Scap.* 5.1–5.

136. *PIR* [2] A 1088; Champlin (1980: esp. 46, 69 f.). On his Asian proconsulship, see Leunnissen (1989: 221) (probably 188–9).

137. Ideally, concluding remarks are required, to answer the question: 'So what?' These pages must, however, serve only as an introduction to the subject, to illustrate how much can be learned about the persecutors and the martyrs. I must here add that I have to my great regret not yet been able to consult Bastiaensen et al. (1987).

Abstract

An account of the persecution of Christians in North Africa during the years 180–212, with emphasis upon the background of the persecuting governors. The sources are Tertullian *and the* Acta Martyrum. *Beginning with the martyrs of Scilli early in the reign of Commodus and the conversion of Tertullian shortly after this, his writings are listed down to 212–213, and the occasions that prompted them are analysed. Extracts are cited illustrating Tertullian's defence of religious freedom and his defiant stand against the cruelties inflicted upon Christians. Not all governors were unjust. Four are named as lenient and some prosopographic details emerge about their careers. The martyrdom of Perpetua and her companions is set out in full against the background of what is known of the acting governor Hilarianus. Tertullian recalls the horrible fates that befall persecuting governors, and predicts the spread of Christianity, through the inspiration given by the martyrs.*

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The end of garrisons on Hadrian's Wall:
an historico-environmental model

by P.J. CASEY*

Despite the best efforts of historians and archaeologists, the end of Roman Britain remains impenetrably obscure. In the north this period has suffered neglect because of the availability of Anglo-Saxon documentary sources, such as Bede, which deal in detail with later historical figures in the area. Thus scholars have been offered richer pastures to browse in than the thin soil of early post-Roman archaeology. The process by which a diocese, with an apparently effective military and civil administration, devolved into a cluster of Anglo-Saxon petty kingdoms cannot be traced in detail, although a number of events which must have contributed are recorded by the imperfect historical sources. We may instance the withdrawal of troops abroad by Constantine III in 406 and their subsequent destruction or incorporation into the imperial army of Italy in 411. The effects of the failure of Honorius to re-establish a Roman administration after the fall of Constantine can also be traced. From incomplete and partial literary records attacks of enemies from overseas and from beyond the northern frontier are known, which culminated in the fatal decision to employ mercenaries who proved to be uncontrollable.

However, archaeological evidence to supplement, or even to overturn this slender historical account seems to be virtually non-existent. Reaction to the events of the late fourth and early fifth centuries and adaptations to the new economic and political climate might be expected to be detectable in the archaeological record. Given the imaginative use to which prehistorians have put their minimal evidence it is surprising that Roman and Anglo-Saxon students have not been more active in venturing to construct social models against which to test historical hypotheses. Whilst it is all too easy to pile a Pelion of theory on an Ossa of speculation, to essay a synthesis of the archaeological evidence with the historical material and to meditate on the sort of society which these data might suggest may serve to open up debate on a neglected topic. It must be emphasised that what follows is hypothetical and that the evidence is certainly capable of more than one interpretation.

A summary of the evidence shows how little consideration this period has received in the past. No cemeteries have been investigated in military contexts and the use of environmental evidence has not yet made much impact on the problems of the continuity of settlement. Of the sites in the study area (Fig. 1) only five of those listed in the *Notitia Dignitatum* have recorded evidence of putative fifth-century activity. Excavation of the Yorkshire signal stations shows that their occupation ended in violence in the period under review.

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Excavations at Birdoswald show a complex series of replacements of major stone buildings by long-lived timber successors which may prove to be of late fourth- or fifth- century date (Wilmott, 1988). During exploration of the south-west angle of the fort wall it was noted that, at a late period, an earth bank had been erected over the defences. This was revetted by a retaining wall on the outer side (Simpson and Richmond, 1933).

At Vindolanda (Chesterholm) evidence suggests that similar earth revetments were kept in commission into the fifth century (Bidwell, 1985). Repairs to the walls in both halves of the fourth century were of poor quality and the face of the walls slumped away from the core. Nonetheless, the walls still stand to a height of over three metres where they were protected by a scree of soil. Noting this, Bidwell suggested that material was deliberately piled against the walls in antiquity in order to prevent collapse. It is suggested that this took place when the resources, or necessary skills, to maintain the walls were no longer available. The ensuing *glacis* defence might have been surmounted by a palisade or a dry stone wall. The excavator concludes that:

> ...in the ultimate phase [the defences] were refurbished in a manner reminiscent of the refortification of some Iron Age hillforts in the fifth or sixth centuries. As to the date of the possible refurbishment, it can be no earlier than the end of the fourth century, since repairs to the wall continued to be made in the second half of that century. (Ibid., 1985, 45–6)

A further piece of evidence suggestive of late activity at Vindolanda is the tombstone of Brigomaglos (RIB 1722), which has been dated, on epigraphic grounds, to the end of the fifth century.

At Housesteads (Vercovicium) it appears that the defences were in a similarly collapsed state in the latest period. The north wall of the fort collapsed outwards leaving only a strengthened internal bank to defend this side of the site (Crow, 1988). No dates have yet been advanced for this phase but since major structural work is elsewhere ascribed to the second half of the fourth century the same arguments as were deployed at Vindolanda may be advanced for the refurbishing of these ramparts in an irregular manner.

The evidence from South Shields (Arbeia) is the most complete. The late Antonine west gate saw constant use in the second half of the fourth century. The causeway across the ditches to the gate was buttressed with a dry-stone wall which sealed sherds of pottery dating to after the late 360s. The gate and adjacent fort wall then fell into disrepair, with stones collapsing into the ditch nearest to the wall face. A new ditch was excavated through this collapsed material cutting off direct access to the gate and there is no evidence for a bridge spanning the new ditch. In the next phase the gate was in a state of collapse, and rubble, including stone window-heads from the towers, was used to create another causeway. Postholes set into the south portal indicate that, though truncated by collapse, it was worth furnishing the gate with a stout wooden door to control entry. Subsequently an inhumation cemetery was established outside the gate (Bidwell, 1988).

This sequence covers a considerable span of time. The phase represented by the cutting off of the gate by a new ditch cannot be directly dated, but since it is preceded by decay it is probably later than a period in which the fort was maintained by regular forces with technical
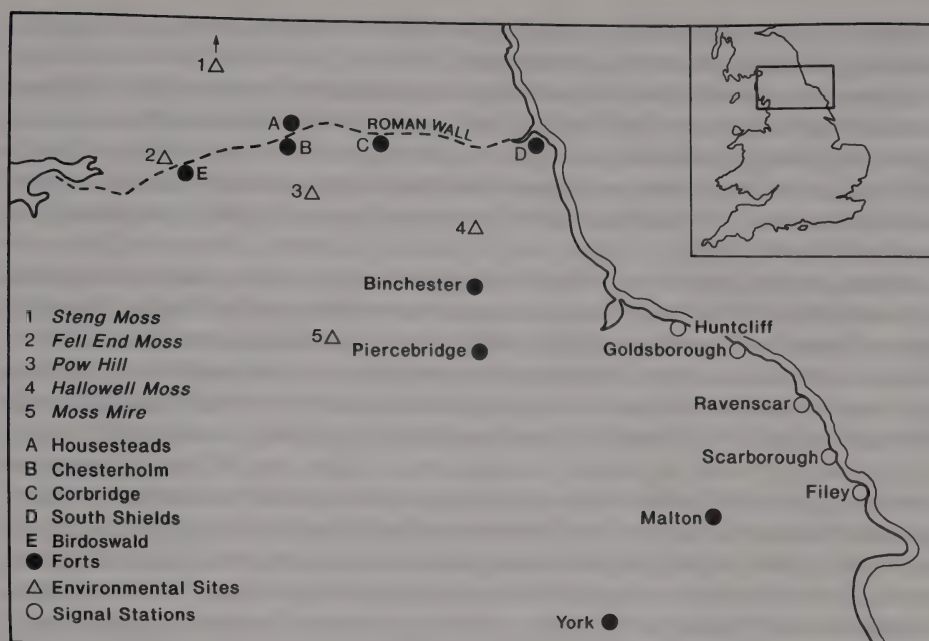
Fig. 1   Sites in NE England with environmental or structural evidence of post AD 400 activity.

skills, and the work may be seen in the same context as the informal repairs carried out at Vindolanda and Housesteads.

Excavations in the south-east quarter of the fort at Piercebridge (probably the Morbio of the *Notitia*), by the late Peter Scott, revealed renewed military activity in the post-Roman period (forthcoming). The ditch fronting the fort shows unmistakable signs of having been re-dug on a very extensive scale after 402, and after an interval long enough for masonry from the wall to have tumbled into the ditches.

The sequence in the ditch is clear. After being cleared out on the reoccupation of the fort in the middle of the fourth century, the main defences once again became a dumping ground for rubbish thrown down from the walls. The coins in this filling include the latest issues to reach Roman Britain. After this date the ditch was recut; wall stones, and the latest issue coins, were stratified below the lowest level reached by the recutting. It is not known whether the re-defence extended around the entire perimeter of the fort but this was a major recasting of the defences and implies, firstly, that a situation existed which created the need for re-defence and, secondly, that a political or social structure existed which could command the manpower and resources to undertake that task.

The *Notitia* garrison of Malton (Derventio) was the *Numerus Supervenientium Petueriensium*, who were posted to the site in the 380s (Casey, 1979). Excavations show a complex series of

adaptations of the defences in the second half of the fourth century or later, culminating in a breaching of the causeway to the north-east gate.

Another extemporised refortification took place adjacent to the south-east gate. In this area a rich, and architecturally varied, *vicus* existed which included houses with mosaics and hypocausts. Through this development a three metre wide ditch was dug, forming a formidable defence, screening approach to the gate itself. Only very late Roman pottery was associated with these phases of re-defence and a fifth-century date for the work is postulated by the excavator (Corder, 1930).

Information from other sites on Hadrian's Wall, and within the study area, is not available but it would be surprising if careful examination on an extensive scale did not reveal very late or post-Roman activity similar to that from Birdoswald, Vindolanda and Housesteads.

Similarly the hinterland forts on Dere Street at Ebchester, Lanchester and Chester-le-Street may be expected to have evidence of continued activity in the light of that from Piercebridge and South Shields. All of these forts were garrisoned until the end of the fourth century, according to the *Notitia Dignitatum,* and coin finds confirm this. It may be noted that Vinovium (Binchester) does not figure in the *Notitia* and that the coin list from the site may be interpreted as being inconsistent with a military presence after the early years of the reign of Valentinian I. Nonetheless, very late stratigraphical phases, which may represent a long civilian presence, have been identified by the excavators (Ferris and Jones, 1979). There is no evidence from the *Notitia* that Corbridge served as a military site at this late period, though the coins extend to the very end of the period of supply. It may be significant that both Binchester and Corbridge are the sites of Anglo-Saxon burials of early date. This is a feature not recorded from the military sites listed in the *Notitia.*

Before considering possible contexts for the activity outlined above, we may review the environmental evidence from the region. A number of sites have yielded pollen data for the Roman and post-Roman period in the north-east of Britain (Fig. 1). The total of sites is still small but the overall picture is a consistent one of extensive clearance from the last decades of the first century BC until the first century AD. Where evidence is available for the later history of these sites, they show a regression to woodland landscape considerably later than what may be regarded as the formal end of Roman occupation in the area.

At Fellend Moss, on Hadrian's Wall, this re-afforestation took place during the 520s. At Steng Moss, in central Northumberland, the date ranged from 480 to 530 and at Hallowell Moss, near Durham City, centred around 550 (Turner, 1979). Further south at Moss Mire, in Teesdale, the local agricultural phase ended at 430 ± 70. Further sites in County Durham, Pow Hill and Thorpe Bulmer, also yield re-afforestation dates in the range 500–600 (Fenton-Thomas, 1990). We will return to consider the implications of this evidence later, but for the present it is enough to note that food resources continued to be available over a wide area for at least a century after the decline of Roman administration.

In reviewing the archaeological evidence for continuity on military sites it has been implicit within the framework of the argument that the inhabitants of these sites were the descendants, or members, of the last garrisons listed in the *Notitia*. The events of the first decades of the fifth century do not appear to have affected these units.

There is no positive evidence that Constantine III took troops from the north in 406 nor is there evidence for their removal at any earlier date in the last decades of the fourth or early years of the fifth centuries. It has been argued that the garrisons of Wales and some of those west of the Pennines were moved out by Eugenius in 393 (Casey, 1989), and Stilicho is credited by Claudian with the removal of troops to protect Italy from Alaric in 402. A fleet and unit which had been stationed at Anderida (Pevensey) was transferred to Gaul (*Not. Dign. Occ.* XLII), but this may not have taken place at a very late date. The most likely source of troops for Constantine's continental campaign is to be found in the field army of the *Comes Britanniarum*. This mobile force of six regiments of cavalry and three of infantry might have amounted to an army of between five and six thousand men (*Not. Dign. Occ.* VII). One of the units of Constantine's expeditionary force which may have been part of the Comes' command was the *Equites Honoriaci*. In his account of Constantine's crossing of Pyrenees Orosius draws attention to a unit of Honoriaci who are picked out for comment because their behaviour towards the Spanish provincials was more barbarous than that of the barbarians themselves (Oros. 7.40).

Given the short time Constantine had to mobilise a force to deal with the sudden barbarian crossing of the Rhine in December 406, removal of the field army made perfectly good strategic sense. Removal of fort garrisons would have been difficult to achieve in an emergency, and would have left the diocese open to attack by the ever threatening Picts, Scots and Saxons. With the defeat of Constantine in 411 the residue of the army in Britain would have been left in limbo, cut off from the empire as a whole and left to make such provision for itself as circumstances permitted.

A parallel for this situation is found two generations later when Majorian (457–61) appointed Aegidius commander of the field army in Gaul. On the death of Majorian, Aegidius, his officers and men, refused to recognise his successors. Aegidius continued to hold command of the now independent army, being succeeded by his son, Syagrius, in 464. The son, elected 'King of the Romans' by his troops, held an enclave of territory at Soissons until conquered by Clovis in 486. For some twenty-five years, then, a Roman army existed in isolation, equipping itself from its own resources. The fate of this army, or of another force in Gaul, is told by Procopius in an account germane to any consideration of the fate of troops in post-Roman Britain:

Now other Roman soldiers, also, had been stationed on the frontiers of Gaul to serve as guards. And these soldiers, having no means of returning to Rome, and at the same time being unwilling to yield to their enemy (i.e. the Visigoths) who were Arians, gave themselves, together with their military standards and the land which they had been guarding for the Romans, to the Arborychi (?Armoricans) and Germans; and they handed down to their offspring all the customs of their fathers, which were thus preserved, and this people has held them in sufficient reverence to guard them even up to my time. For even at the present day they are clearly recognised as belonging to the legions to which they were assigned in ancient times, and they carry their own standards when they enter battle, and always follow the customs of their fathers. And they preserve the dress of the Romans in every particular, even as regards their shoes. (Procop, *Goth.* V.12.17)

A similar situation may have existed in Britain after 411. Unfortunately, as yet, knowledge of the late phases of the legionary fortress at York does not suggest conclusively that a command structure for the North could have been based on the seat of the *Dux Britanniarum*. Nevertheless, the south walls of the fortress were refurbished with projecting towers in the late third or early fourth century, and remained a key point of the city's defences into the mediaeval period. Command structure or not, individual forts would have eventually realised that no orders were coming through, no officers were being replaced and that no pay and donatives were arriving. Even in periods of stability, individual units might have been out of touch with current developments. In the reign of Diocletian the garrisons of Egypt were several years behind in receiving their pay, a situation only rectified by the imminent arrival of the emperor on a state visit (Skeat, 1964). Alarm may have not been felt for a number of years and, even when it was, units in remote stations had little choice but to stick it out.

If it is accepted that units may have remained in station, a number of questions may be posed. What might the nature of these garrisons have been and what may have been their relationship to the rural population? Against what threat was the re-defence of the forts undertaken? Can any chronology be established for events?

The size of the late Roman garrisons in the north of Britain is uncertain. In many forts traditional barracks, designed to house eight men, were replaced by so-called 'chalet' barracks in the late third or fourth centuries (Daniels, 1980). A chalet row occupied much the same space as the barracks which they replaced but with a reduced number of units of accommodation. It is agreed that this change indicates a decrease in the size of fort garrisons, but there is no agreement as to the degree of reduction. Assuming a reduction in living space from ten units to six, with the replacement of barracks by chalets, and the use of the buildings exclusively by soldiers, Wilkes has proposed a reduction in unit size of 40 per cent (Wilkes, 1966). By contrast Daniels regards each chalet as having been the residence of a military family with one or two members serving as soldiers; this view reduced units by up to 90 per cent of the traditional strength (1980). Documentary evidence from the Eastern empire suggests that as early as the reign of Diocletian a 500-strong infantry unit had been reduced to 164 and a similarly sized cavalry unit to 118 men, reductions of 66 per cent and 75 per cent respectively (Duncan-Jones, 1978). By the the late fourth century the legion probably mustered 600 men. Even at these levels of reduction a considerable force, in contemporary terms, was still located on the Wall and in its hinterland.

What relationship might garrisons have had with the rural population? The region enjoyed an improvement of agriculture in the pre-Roman period, evidence of a native British stimulus, but settlements of the Roman period show little sign of increased wealth from this enterprise, suggesting that Roman intervention inaugurated a regime of pre-emption of produce, which benefited the army rather than the producer. The absence of villas, such as those that developed from native farmsteads in the south, may be evidence for the imposition of an economic regime in which produce was directed at a low fixed price to a coercive military market. With the transition, in the fourth century, from a cash-based tax system to one in which produce was levied, even the minimal sums paid for goods by the authorities dried up. Since the army was paid with the produce surrendered in assessed levies it can be expected

that, in these circumstances, the symbiotic relationship between the garrisons, their dependents and the local farming community would have been reinforced. It may not, of course, have been a happy situation. With the decline of central authority the link with the residual military population might well have changed from one of coercion to one of mutual dependence, a measure of peace and stability being provided by the military element based upon the forts.

Turning to the political situation which may have faced post-Roman garrisons, it is clear that a military threat existed from the evidence of re-defence, especially from the provision of new ditches and the isolation of gates. That the threat was from the Scots and Picts is clear from Gildas who records, albeit in a garbled manner, a number of incidents: two in the period of Roman administrative control and two in the post-Roman period. Though it is unfashionable to give credence to Gildas it is worth examining whether anything in the archaeological record supports or contradicts his narrative. Of the later episodes he records: '…there eagerly emerged from the boats that had carried them across the sea the foul hordes of Scots and Picts…they seized the whole of the extreme north of the island from its inhabitants, right up to the wall' (Gildas, 19.1).

There follows an account of defences and towns being abandoned under the onslaught, as famine struck and civil war broke out in the struggle to control scarce food supplies. An appeal was made to Flavius Aetius, the *Magister Militum* in Gaul, for assistance, which was not forthcoming. Apparently sated with plunder, the Scots and Picts returned home and a period of prosperity prevailed. This prosperity led, in Gildasian terms, to the onset of moral degeneracy. The latter was directly responsible for dire events, including 'the imminent approach of the old enemy, bent on total destruction and (as was their wont) on settlement from one end of the country to the other' (Gildas, 22.1).

A further consequence of the 'moral decline' was the decision to invite Saxon mercenaries to come to Britain to deal with 'the old enemy'. The chronology of these events is uncertain but the one apparently fixed point can be reviewed.

The appeal to Aetius 'thrice consul' appears to date between 1 January 446, when Aetius entered his third consulship, and his death in 454. The text of the appeal in Gildas does not, in fact, mention Aetius but 'Aigitius', a confusion with Aegidius, the *Magister Militum* whose career was reviewed above. Aegidius did not hold three consulships, so an amendment to the reading 'Aetius' is agreed by all scholars.

To confuse things further there was not one Flavius Aetius but two, who were exact contemporaries. Both held the consulship and both were victors over Attila the Hun. The second Flavius Aetius was the *Comes Domesticorum et Sacrum Stabulorum* in the East. In 452 he led an expedition, sent by the Emperor Marcian, against the Huns on the Danube when Attila was campaigning in Italy. The Huns were routed, Attila withdrew from Italy and Aetius was rewarded with the consulship. This Aetius was well known in the West since he appears on a consular-dated inscription in Spain. There might then have been a need to differentiate between the two anti-Hunnic heroes. In these circumstances it might be possible to read the address of the Appeal as a later gloss serving to define the recipient as the Aetius who held three consulships rather than the Aetius who held only one, and the superscription of the Appeal may well define the person addressed, rather than providing a date (Casey and Jones, 1990).

If this revisionist interpretation removes the stumbling block of the date of the Appeal we may shorten the chronology of some of the events in the post-Roman period to accommodate the evidence of the Gallic Chronicle which records that a significant part of Britain was under Saxon control by 441 (Jones and Casey, 1988). Taking a minimalist view we might look for recorded dates at which Aetius was in Gaul with an army, and thus able to be contacted and to intervene. These dates are 429, 432, 436, 442, and 444. On each of these occasions Aetius was fully occupied in suppressing barbarian forces, or social unrest, in Gaul. It is worth reiterating that the Appeal stemmed from the aggression of the Picts and Scots and that Saxons were only invited to Britain after Aetius failed to respond.

Having sought to establish that, in the early part of the fifth century, the Scots and Picts were the immediate enemies with whom the post-Roman population had to contend, we may return to the events in the North and consider the narrative of Gildas in the light of archaeological evidence.

There appears to be fifth-century recasting of fort defences, but are these activities ascribable to Pictish activity? John Mann perceptively called the east Yorkshire coast, with its signal stations perched on the high cliffs, the 'Pictish Shore' (Mann, 1977), a description which is all the more pertinent if the towers really were built by Magnus Maximus after his Pictish campaign (Casey, 1979). The towers might be seen in relation to Pictish raiders sailing down the east coast, circumventing Hadrian's Wall and initially linked to naval forces. Later they may have served simply as early warning stations for the land-based population. If this is so, the destruction of the signal stations might have heralded an intensification of raiding. The question, then, is when did the coastal defences collapse? Here a problem fundamental to all studies of Late Roman Britain must be faced: the only intrinsically dated material is the coinage and this ceased to arrive in bulk with the copper issue of Honorius bearing the reverse legend *Salvs Reipvblicae*; and this was replaced in 403 by an issue not found in Britain.

Three of the Yorkshire sites show signs of violent destruction. At Goldsborough the skulls of a woman and two men were found in the well, and in the south-east corner of the tower were found two male skeletons with 'numerous sword cuts' on the skulls (Hornsby and Laverick, 1932).

At Huntcliff the well produced the skeletons of 14 decapitated men, women and children (Hornsby and Stanton, 1912). Filey produced no human remains but the site appears to have been destroyed by fire and the structure deliberately slighted (Cortis, 1857). Scarborough is unpublished but the evidence available suggests an attack resulting in destruction.

It is stretching Gildas too far to associate this evidence directly with his account of events in the North immediately prior to the Appeal to Aetius: 'So they [i.e. the Scots and Picts] seized the whole of the extreme north of the island from its inhabitants, right up to the wall. A force was stationed in the high towers to oppose them, but they were too lazy to fight...rotting away in their folly' (Gildas, 19.1).

Despite Gildas's claims that the motive for Pictish raiding was conquest, the reality appears to be that the incursions were opportunistic. North of Hadrian's Wall a situation may have existed which served to divert Pictish activity further south. Following the abandonment of the outpost forts in the early fourth century (Casey, 1978), a re-awakening of native

political institutions which burgeoned throughout the fourth and fifth centuries may be envisaged. Nucleated hillforts were reoccupied and prestige goods were acquired at the central site of Traprain Law (Curle, 1923). State formation developed, initially under tribal auspices, as appears to be the case with the Votadini and, later, after internal struggles alluded to by Gildas, in the shape of some sort of unified British entity. The Britons furthest from the traditional Roman frontier may have been sufficiently strong to prevent overland, southward, expansion by the Picts. Such a territorial block at a time when Pictland was in a dynamic and active phase could have encouraged expansion by sea to bypass the problem. Denied access to the south-east of Britain by the presence of Saxons possibly at an earlier date than hitherto allowed, the north-eastern military zone may have experienced intensified Pictish activity.

It is in the light of this model that the destruction of the Yorkshire coast signal stations and the attempts to strengthen the decayed inland fortifications of the northern region may be viewed. With the exception of a single burial at Castle Eden, Durham, of the mid fifth century, the archaeological evidence for Saxon presence in the area seems to be late fifth to mid sixth century in date (Miket, 1979), about a century after the collapse of formal Roman authority in the region. It is during this period that the developments outlined in this paper may have taken place.

Against an uncertain political background, and early in this period, hoarding of valuables may have taken place. The presence of three hoards close together within the study area is significant. The Richmond hoard is reported as containing six hundred coins, presumably silver, and the Wilton hoard one gold and seventy-nine silver coins (Archer, 1979). The Whorlton (Guisborough) hoard may claim to be one of the largest deposits ever recovered, amounting to several thousand silver coins as well as a large silver dish, spoons, buckles and hacksilver (Burnett, 1979). It is worth noting that coins of the type discussed here have not been found in Anglo-Saxon graves nor were they reused in jewellery, suggesting that they were no longer available when contact with Angles or Saxons was made.

When Anglo-Saxons did appear on the scene their presence need not have been unwelcome, especially in an area where Pictish pressure had been great. Of the traditional military sites only Binchester and Corbridge, neither of which appears in the *Notitia Dignitatum,* have Anglo-Saxon burials. This slender evidence may support the view that sites listed in the *Notitia* still retained, in some attenuated manner, a Roman status and population. Rather than succumbing to Anglo-Saxon attack, forts may have been abandoned when a fusion of the population took place, which would have made them both inconvenient as habitation sites and obsolete as military installations. This model is compatible with the environmental evidence, which shows a mid fifth to mid sixth century abandonment of peripheral agricultural zones which may have been accompanied by easier access to the more fertile soils of the newly pacified coastal area.

The situation appears to have been different further north. Here the British kingdoms seem to have resisted both Pictish advance and Anglo-Saxon expansion. Possibly the northern Britons were better able to manage their own affairs, resist the Picts and have no need of Saxon intervention on their behalf; in the south lingered the last elements of the Roman system as it had existed for three centuries. To the north there was a new or newly revived

social and military system; to the south a decaying Roman one retaining the fixed-point defensive strategy of the late Roman world. This system was embedded economically in the countryside which supplied the necessities of the troops and had as its basis a recognisable social unit, that of the regiment. The refortification of individual fixed points is eloquent testimony to the inheritance of the paralysing effects of late Roman political developments which strove to create ever smaller governmental units, devoid of local decision-making ability and dependent on a centralised executive body approachable only through a long and corrupt chain of bureaucrats.

Abstract

The end of Roman Britain is little understood, as the archaeological evidence is still partial, especially in terms of chronology. Along Hadrian's Wall, however, despite the probable withdrawal of the field army in 406, a reduction in the size of barracks and lack of cash payment, the Roman army appears to have remained in some capacity, as levying of produce may have been possible, and as renewal of defences seems to have occurred in the fifth century.

The main threat in the early fifth century appears to have been from naval raids by Picts and Scots, evident in the violence with which at least some signal stations on the east coast, and south of the Wall, came to an end. The re-emergence of a native British political social structure north of the Wall may have protected its forts, which survived until a fusion with the Anglo-Saxon population (probably intervening against the raiders) occurred from the mid fifth to mid sixth centuries, as they no longer proved convenient as habitation sites. This is suggested by the environmental evidence, as a regression back to woodland did not occur until about a century after the end of Roman Britain.

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The Role and Purpose of the British Museum

by J. RANKINE*

We are pleased to include in this issue the first of what is hoped will be an annual lecture series on Museum Studies at the Institute of Archaeology. It is intended to invite a distinguished figure from the Museum (or Heritage) world to speak on a subject of his or her choice at an informal gathering toward the end of the Autumn Term. The 1992 lecture was delivered by Miss Jean M. Rankine, Deputy Director of the British Museum, a graduate in German and Old Icelandic, and a Fellow of University College London. The occasion proved both enjoyable and stimulating and we are most grateful to Miss Rankine for providing so promptly a text which, though slightly revised, is in essence the lecture as delivered on 10 December 1992.

J.J.W.

The British Museum was founded by Act of Parliament in 1753 when Sir Hans Sloane bequeathed his vast collections to the nation. In the breadth of his interests and his learning, Sloane was a true eighteenth century man, a representative of the Age of Enlightenment. He was a doctor of medicine and collected books and manuscripts on every branch of science, as well as plants, fossils and minerals, and zoological, anatomical and pathological specimens. His interests in the natural sciences were equalled, if not surpassed, by his historical and antiquarian interests, and in the course of his long life—he lived to be 93—he acquired an amazing assortment of antiquities of the classical and biblical lands, oriental curios, ethnographical specimens and even drawings by Dürer, Holbein and other masters.

Sloane bequeathed his collections to the nation in order that they might 'be kept for the use and benefit of the publick, who may have free access to view and peruse the same'. It was both this Act and his example which inspired the concept of the British Museum as a universal museum, one which would cover every field of human knowledge and assemble 'the natural and artificial productions of the whole world'. The primary purpose of the Museum was to further scholarship and scientific enquiry but from the beginning its treasures were to be made more generally available. The regulations drawn up in 1758 state that it was 'chiefly designed for the use of learned and studious men, both natives and foreigners, in their researches into the several parts of knowledge...yet being a national institution it may be judged reasonable that the advantages accruing from it be rendered as general as possible.'

Our founders' ideals remain more-or-less intact to this day. The British Museum is a storehouse both of objects and of knowledge about those objects which is used by scholars

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81

from all parts of the world and its exhibition galleries are visited by between five and six million people every year, of whom perhaps sixty per cent come from overseas. The collections are drawn from every part of the world and cover the history of man from the earliest time to the present day, but the fields of human knowledge and human endeavour have grown and multiplied to such an extent in the last two centuries that it has not proved possible to retain the tangible evidence of all of them under one roof or one administration. The National Gallery, the Natural History Museum and the British Library all trace their origins to collections hived off from the original British Museum, while there are at least four other national museums dealing with aspects of human endeavour which complement and supplement those encompassed by the British Museum as it is today. However, none of them existed in 1753, there was no such thing as a national museum anywhere in the world and the British Government of the day was rather tentative about setting it up. Then, as now, money was the big problem, and there was no precedent for a direct charge on the Exchequer for anything of the sort, so Parliament decided to raise money by means of a lottery. This was reasonably successful for the new museum, in that sufficient funds were raised to set up, house and provide a very small endowment; and it was very successful for the promoters, who ran off with half the proceeds. The resulting scandal had a lot to do with the prohibition of parliamentary lotteries thereafter. However, we seem to have come full circle. If the National Lottery Bill does pass successfully through Parliament this session, the Treasury if not the Government will certainly view it as a hedge against dipping into the Exchequer.

Montagu House was chosen as the home for the new museum. This was a pleasant, if rather decrepit, seventeenth-century country residence on the site of the present Museum, which was then on the outskirts of London. At £10,000 it was felt to have a considerable advantage over one of the other properties under consideration, Buckingham House now Buckingham Palace, for which the asking price was £30,000. However, it also cost rather more than £10,000 for repairs and refurbishing before the collections could be moved in. Once established the Museum grew apace and within fifty years of opening Montagu House was bursting at the seams; by the 1820s it was decided that it should be demolished piecemeal to make way for a new purpose-built museum, the nucleus of the buildings we have today. The first part of the new building to go up was the long gallery designed to house the Kings Library, the library of King George III which came to us in 1823, and above it the national collection of paintings. This upper gallery was never put to its intended use as the Government decided to establish the National Gallery as a separate institution in 1824 (there was an aesthetic lobby in Parliament which did not like the idea of the great works of Raphael being placed in the same edifice as collections of animals and fossils).

The fossils and the stuffed animals remained with us for another half century until the natural history collections were moved in the early 1880s to the splendid building in South Kensington designed for them by Alfred Waterhouse and became the British Museum (Natural History).

Another ninety years passed and we split in two yet again when, with the passing of the British Library Act 1972, responsibility for the Manuscript and Printed Book collections was transferred from the British Museum Trustees to the newly constituted British Library Board.

The new institution was to occupy a new building, originally planned as an extension to the Museum, to the south of Great Russell Street, which was expected to take about ten years to complete. Ten years later, in 1982, the foundation stone was laid for the new BL building at St Pancras which ten years later still, in 1992, is still insufficiently near completion to take in a single book. The move from the Bloomsbury building to the new quarters at St Pancras was always planned in several stages, so even if Stage One of the new building is available within the next couple of years it seems unlikely that the two institutions will finally disentangle themselves before the end of the century. Meanwhile, the logistics of running two institutions in a building that is inadequate for either of them get ever more complex.

It is of course a magnificent building, perhaps the greatest neo-classical edifice in this country and as much part of our heritage as the collections which it contains. After twenty-five years I still feel a sense of privilege as I drive in through those gates each day—but it is also the source of many current problems. The original concept, the 1823 plan, of the architect, Robert Smirke, was simple enough—long, two-storey galleries on three sides of a central courtyard, and the fourth side embellished by a grand neo-classical portico. But the collections grew faster than the building during the next thirty years and in 1852 Edward Hawkins, the Keeper of Antiquities, complained that

> although the building is not yet finished, scarcely a room remains as it was originally constructed, great alterations have been found necessary in almost all; the building is surrounded by inconvenient and unsightly excrescences, and it may be asserted with truth, that Europe cannot show any building so ill adapted for its intended purpose as the British Museum.

And what was true of the first thirty years remained true for the next one hundred and forty. The most recent external addition was a new wing erected in the south-west corner in the 1970s, while internally in the 1980s three floors were constructed in a corner where once there were one and a half, to provide a new home for the Coins and Medals Department over a gallery of Greek antiquities and an entirely new suite of galleries, with a students room and study areas, was created for the Japanese collections in the attic space above the Prints and Drawings gallery. We have inherited a bewildering complex of galleries, offices, studies, workshops, stores—on many different levels. The three hectares of roofs are at many different heights, from the single storey, lean-to, extensions a few feet above ground level to the lofty dome of the Reading Room, and the cellars go down three levels to a water-filled tunnel, popularly believed to be a tributary of the Fleet river (perhaps because that is more romantic than acknowledging that it is an offshoot of the great London sewage system).

It would be difficult enough to keep this disparate collection of structures weather-proof and in working order even if we didn't have the wear and tear of several million pairs of feet tramping through every year—this year looks like being a record six million—and statutory obligations to look after the owners of those feet. As soon as a building is open to the public, (which the BM is on 359 of the 365 days of each year) there are regulations about fire exits, widths of doorways, numbers of stairs, levels of lighting, etc., none of which hampered the original architects. Neither were the creature comforts of staff or visitors a major concern; so

many of the older galleries are still without heating in winter, while the glazed roofs installed to maximise the use of daylight send the temperatures soaring in summer. Measures taken to counteract these defects come up against the limitations of what one is allowed to do to a Grade I listed building, and the conflict between the requirements of the laws which protect the building and the laws which protect the people who use it (not to mention the conservation of the Museum collections in it) are difficult and expensive to reconcile.

Some years ago there was a great furore over the redecoration of our Front Hall. The Victorian Society and other members of their 'conservationist' lobby said that we should restore it to its 'original glory' and reinstate a mid-Victorian decorative scheme of dark reds, greens and a dark blue ceiling with gold stars on it, while the Trustees and Director intended to replace the grubby cream and yellow with light, clean shades of grey and stone colours. Whether one prefers multi-colours and stars or light grey and stone is entirely a matter of taste, and tastes differ. The question of what constituted the 'original glory' is equally open to debate: Robert Smirke planned the Front Hall in the 1820s and he is on record as having preference for light, plain colour schemes, but he retired in 1846 when the Hall was still in process of construction. The riot of dark colours is much more the taste of the later generation represented by his younger brother, Sydney Smirke, who succeeded him as museum architect. There were, however, two overriding practical considerations. First, the polychromatic scheme would have cost twice as much and taken twice as long to execute. As it was, the grey scheme cost nearly £100,000 and, even if another £100,000 could have been found, the Museum had many other priorities for spending it. Second, the Victorian decorative scheme included no provision whatsoever for light-fittings, which was all very well when one could throw the public out as soon as daylight faded and need not care very much whether they fell over each other in the dark on the way. A century of electricity (in 1891 the BM was the first public building in London to instal electric light in its public galleries) has raised public expectations and today's visitors may sue the management if they trip and twist an ankle.

This conflict will undoubtedly emerge in even sharper focus in the course of the next few years as the Museum falls heir to the space vacated by the British Library. The Library at present occupies forty per cent of the floor area—in terms of crude square metres—which sounds like a great deal of extra space. But there are constraints. The King's Library was built to house the Library of George III given to the nation by George IV, and the suite of galleries leading to it were created in the same style. The Reading Room was the greatest of the 'excrescences' deplored by Hawkins; it was dropped into the central courtyard, obscuring the fine elevations, because the Reading Room facilities planned twenty years earlier were hopelessly inadequate by the 1850s. In a few years' time the Museum will have neither the books to suit the shelves nor any use for the readers' desks, so what to do? In neither case was there any original intention of admitting members of the general public, yet both are among the most prestigious public rooms in the country.

In some ways the non-public spaces present even greater difficulties. More than sixty per cent of the total Library space is purpose-built book storage, the bookstacks built round and possibly propping up the great Reading Room. The bookstacks are quite unsuitable for the storage of any part of the Museum's collections—they are not even very good for books,

which is one of the reasons why the Library is moving. We need to adapt the space for Museum purposes, for an education centre, exhibition galleries with preparation areas and drawing offices, a new enlarged bookshop—these are only some of the functions we hope to accommodate in the Library space. These functions all need more headroom than bookstacks, and higher ceilings per floor mean fewer floors altogether, unless one can go higher—not possible in this case because it might spoil views of the dome—or lower, also not possible in this case because we would end up in the sewer.

However, these are the Museum's problems and there is some chance that the Museum will find ways of solving them. Up until four years ago, the Government, through the DoE PSA looked after the fabric of the buildings of the national museums and galleries, so that every proposal put forward by the Museum was second-guessed and sometimes altered out of all recognition by civil servants whose interests lay elsewhere. On 1 April 1988 responsibility for works and maintenance was transferred to the Trustees of each of the national museums, and the funds previously provided to the PSA to maintain the national museums and galleries estate are now allocated to the individual institutions as part of the annual Grant. This does not mean there is no scrutiny from Whitehall, but at least it is restricted to one department, the Department of National Heritage, whose predecessors, The Office of Arts and Libraries, were involved anyway and who frequently had yet a third set of interests, which did not always harmonise with those of the PSA. The BM now has its own Works Department employing architects and engineers, and a work-force of well over a hundred technicians and craftsmen with a wide variety of skills, from monumental masons to the precision skills of locksmiths and watch-makers, and a similar number of semi- or unskilled labourers, handymen and cleaners. It is a sign of the times that there are also two full-time Health and Safety Officers on the strength of the Department.

A museum building however grand, historic, listed and worthy of attention in its own right is still secondary to its contents—the essence of a museum is its collections and it must primarily be the needs of the collections and the use of the collections which determine how they are housed and the quantity and quality of staff needed to look after them and to make them available for use. The first responsibility laid on the Trustees by successive Acts of Parliament is to keep the collections safe and to that end we employ about three hundred and fifty security staff. All the museum premises are manned 24 hours a day on a 4-shift system.

The BM also employs some thirty photographers, sixty conservators, thirty scientists, designers, editors, education staff, librarians, information staff, administrators, accountants, auditors, clerical and secretarial staff, all with skills and qualifications in their particular professional fields. Most came to the BM with those qualifications, certainly those who joined the staff within the last twenty years were recruited on the basis of their professional skills, knowledge or qualifications and have adapted them where necessary to their specific role in the Museum with the help of in-service training—mostly in-house and on-the-job. Many have worked in other areas, some will move on to work elsewhere, outside the museum world altogether. The one kind of animal peculiar to the museum is the specialist academic staff, the curators, of whom there are about one hundred and twenty, all of whom are recruited on the basis of their academic qualifications, usually at post-graduate level, in subjects

relating to some aspect of the collections and the majority spend their entire career, or the balance of it if they have worked elsewhere first, in the British Museum. Many of them are, or will become, recognised international authorities in their fields. As I am sure most of you know, the collections are divided between ten curatorial departments:

Coins and Medals holds well over half a million items. It is one of the three largest coin cabinets in the world and is the most representative of world coinage and currency as a whole (650,000 items).

The *Prehistoric and Romano-British* collections are drawn from the Palaeothic and Mesolithic Age throughout the world, from the Neolithic, Bronze and Iron Ages in Europe and from Roman Britain (2,500,000 objects).

The collection of *Greek and Roman Antiquities* (85,000 objects) is one of the finest in the world, covering almost every aspect of Greek and Roman visual art, from the Bronze Age civilisations of the Cyclades and Crete of the third millennium BC down to the Late Roman Empire, except for the province of Britain.

The *Egyptian* collections number some 75,000 objects, illustrating every aspect of ancient Egyptian civilisation, and are the largest and most comprehensive outside Cairo.

Western Asiatic Antiquities covers the cultures of the Ancient Near East from the earliest agricultural settlements about 7,000 BC, until the advent of Islam in the seventh century AD. The collection of Assyrian sculpture is rivalled outside Iraq only by that of the Louvre (300,000 objects).

Oriental Antiquities takes off where *Western Asiatic Antiquities* draws its boundaries both in time and space; it covers the Near East and Persia from the rise of Islam and the rest of Asia from the Neolithic period onwards. The collections from the Indian sub-continent are the most comprehensive in the West. We have the best collection of Chinese porcelain in Europe and the finest holding of Islamic pottery outside the world of Islam (150,000 objects).

The *Chinese, Indian and South Asian* collections have just been redisplayed to wonderful effect in the gallery which will henceforth be known as the Hotung Gallery of Oriental Art in acknowledgement of the gift of £2 million from Mr Joseph Hotung of Hong Kong which paid for the refurbishment. It was opened by the Queen just a month ago.

Japanese Antiquities is the youngest Department created only five years ago to provide the infrastructure to cope with the ramifications of raising £6 million (£5 million of it from Japanese sources) to build the new Japanese galleries (25,000 objects).

The Department of Ethnography holds enormous collections derived from the indigenous peoples of Africa, Australia and the Pacific Islands, North and South America and some areas of Europe and Asia. (Various ancient as well as recent and contemporary cultures are represented.) One of the great strengths of this Department of the BM is that its early collections include material from non-western societies prior to extensive European contact. I believe some of you visited the Museum of Mankind last month and heard something of the

Department's current and future programme from Dr Durrans (300,000 objects).

The Print Room of the British Museum contains one of the world's great collections of European prints and drawings. The series of woodcuts, engravings and etchings, from the early fifteenth century to modern times, is as representative as any in existence; and the collection of drawings includes all European schools from the fifteenth century onwards (2,500,000 objects).

Finally, *Medieval and Later Antiquities* is concerned with the art and archaeology of Europe and certain other Christian and Jewish cultures, from the Early Christian period to the twentieth century (700,000 objects).

Like the collections, the staffs of each Department vary greatly in size. Each is headed by a Keeper, assisted by a number of middle-range and junior academic staff and other support staff, 2 + 6 in the case of JA, 17 + 23 in MLA. The academic staff are responsible for cataloguing, publishing and displaying the material in their care, answering enquiries and recommending to the Trustees that objects be bought or accepted as gifts for their particular part of the collections. Many of them also devote part of their time and energy to fund-raising in support of these activities—but more of that later. Each Department has an acquisition policy, which is reviewed annually by the Trustees when the Keeper presents his or her annual report, but the arguments advanced in support of these recommendations for acquisition can all, according to one of our long-serving Trustees, be reduced to one of two basic essentials. It is either the case that:
a. the British Museum has the best collection in the world of this kind of object and therefore the one currently on offer belongs with them; or
b. due to some extraordinarily unfortunate combination of circumstances the British Museum has so far failed to acquire an example of this kind of object and we should therefore seize the opportunity to fill this gap.

Whichever it is, the case is put forward with all the eloquence at the Keeper's command, for needless to say there is keen competition for the inevitably limited resources of the Purchase Grant. At £1.4 million our Purchase Grant in cash terms is lower this year than it was eight years ago and its purchasing power on the international market is further weakened by the state of the pound, but we don't see this as the end of the world. Of course we are outbid in the saleroom if we happen to come up against any of the handful or so really wealthy American or Japanese museums or the much larger number of private collectors keen to invest their dollars and yen in 'great art', but market forces apply to the arts as to anything else and the wise person does not buy at the top of the market. The great strength of the British Museum's collections rests not on the occasional acquisition of one or two highly prized and highly expensive objects, but on a long tradition of seeking out the unrecognised, under-valued or unfashionable, of buying when the price is low and gathering up what would otherwise be thrown away. We were able to acquire the great bulk of our fabulous collections of classical, Egyptian and Near Eastern antiquities in the late eighteenth and nineteenth centuries because the Ottoman Sultan and his secular officials were not interested in the relics of the civilisations which preceded them. The *firman* authorising the removal of the Lycian

marbles from Xanthos by Sir Charles Fellowes refers to material 'of no use'. Their religious colleagues took an even dimmer view of pre-Islamic antiquities (material from 'the time of ignorance'), regarded most infidel art as sacrilegious and were therefore happy to see their communities rid of it. The peasants and artisans thought Europeans like Layard and Belzoni mad to take so much trouble to remove old stones out of the desert, but were quite happy to earn some infidel gold while assisting their efforts.

In the early years of this century Campbell Dodgson, Keeper of Prints and Drawings from 1912 to 1932, was collecting Post-Impressionist French prints when they cost only a few pounds each. One of the items in our 1978 exhibition of French Lithographs was a Toulouse Lautrec which we bought in 1977 for £17,250, more than Dodgson paid for most of the other 174 pictures put together. We started buying Japanese prints 100 years ago when they cost next to nothing—the print wasn't recognised as an art-form in Japan—but in 1985 the citizens of Tokyo flocked to see an exhibition of Japanese prints, paintings and illustrated books lent by the British Museum. All the items in that exhibition would now fetch substantial prices on the international market, so we have turned our attention specifically to the modern and contemporary field where prices are still low and collectors, including Japanese collectors, are still unsure of their taste.

It is the same story with masks and carvings collected by travellers in Africa and the Pacific, which were once regarded as no more than grotesque curiosities and were obtained by our discerning predecessors for only a few pounds or given to us by relatives and descendants of the travellers wishing 'to get those nasty things out of the house'. As museum objects the anthropologists and ethnographers were always interested in what they could tell us about the societies which produced them, but as art objects they would now fetch six-figure sums in the saleroom. Dr Durrans no doubt explained that the British Museum now tends to concentrate on acquiring documented material on the life and technologies of societies which are fast disappearing. I imagine you saw the Day of the Dead, the Skeleton at the Feast, exhibition for which much of the material was collected or commissioned during three fieldwork expeditions to Mexico, and some exhibits are still being made by the various craftsmen-in-residence who have demonstrated their arts as a feature of the exhibition and the accompanying educational programme.

Of course we do sometimes go after objects with very large price tags, many of which we have an obligation to try and acquire under the 'national heritage' banner. This may be a Private Treaty sale, where there is a small tax incentive for an owner to sell something that is of importance to the national heritage to a national collection, or it may be an opportunity to acquire an important heritage object which has been sold abroad but for which an export licence has been withheld. In either case there is a good chance that the Trustees of the National Heritage Memorial Fund will be sympathetic to a request for help with the purchase. Whether that sympathy can be translated into a cheque depends of course on how many other calls there are on the Fund in the same year; they were able to help us this year with the purchase of both the Towneley papers and the Armada Service, but only by spreading the cost of the latter over two financial years. If the hoard of Romano-British silver that turned up at Hoxne the other day comes up for acquisition and if the price is anything like the figures

bandied around in the press both the BM and the NHMF will be hard-pressed to cope—especially as the NHMF grant for next year has been reduced from £12 million to £7 million.

We have always been fortunate in our friends, both individuals and institutions. Bodies like the National Arts Collections Fund, The Pilgrim Trust and the Goldsmiths' Company have helped us time and again to acquire objects which would otherwise have been beyond our purse, and there is our Friends Association, the British Museum Society, who buy us things or contribute to the cost of others. There is an American lady who gives us a regular Christmas present and a Canadian, J.P. Brooke-Sewell, who bequeathed a substantial fund for the purchase of oriental antiquities in the 1950s. George Bernard Shaw made us one of his residuary legatees so every time you buy a ticket for 'Pygmalion' or 'My Fair Lady' a small fraction of the ticket money comes to the British Museum.

Although some of these benefactions were entirely unsolicited it would be naïve to pretend that this all happened by chance—my colleagues and their predecessors have always included among their number individuals who were talented at soliciting gifts in cash and kind. However, it was not overtly or specifically part of the job, and such matters tended to be handled very discreetly. In recent years successive governments have made it a condition of continuing goverment support that the national museums and galleries also find other sources of funding. So, on the American model we have created higher levels of supporting membership, Associates and Patrons who undertake to contribute at least £100 and £1,000 p.a. respectively, and we have set up an American Friends of the BM registered in Washington DC so that the US taxpayers may claim US tax relief on their charitable gifts to the BM. The Greek and Roman Department has set up a group of Friends, mainly American, called the Caryatids. Japanese Friends of the BM will be inaugurated next year and there is scope for others. It is now the case that those who are known to have wealth or influence who receive an invitation to dinner at the British Museum should consider whether they can afford to accept—just as in the US the words 'I would like you to meet Mr X—he is a Trustee of our museum' mean 'Reach for your cheque-book or make good your escape.'

Gifts of money are always welcome but by far the greater number of important benefactions have been in kind rather than cash. From its foundation the Museum has been the recipient of generous gifts and bequests from all kinds and degrees of benefactor and we are very fortunate that this tradition persists even today. Such benefactions range from the magnificent collection of classical and Near Eastern bronzes, coins, gems and Old Master drawings bequeathed by Richard Payne-Knight which came to the Museum on his death in 1824, to the vast collection of glass bequeathed by Felix Slade, founder of the Slade School of Art, in 1868, to the Waddesdon Bequest, one of the richest Renaissance collections in the world, left to us in 1898 by Baron Ferdinand de Rothschild. Since the last war we have been given the treasure from a Saxon king's ship burial at Sutton Hoo in Suffolk; the Ilbert collection of clocks and watches in 1958; 1188 items of jewellery given by Professor and Mrs Hull-Grundy between 1978 and 1983 together with their bequest of over 500 Japanese netsuke which came to us on their deaths, within a week of each other in August 1984. Also the 580 pieces of Islamic pottery bequeathed to the Museum by Miss Edith Godman in 1982, and earlier this year the Schmitt-Meade collection of Japanese tea-wares and Tibetan art and

the Poulsen-Hansen collection of Korean antiquities, and much else. Each time our Trustees meet, which they do 9 times a year, they have before them lists of gifts offered to the Museum since their last meeting. The last meeting was fairly typical: the Keeper of Coins and Medals reported that the British Museum Society had bought for the Museum, at a cost of £25,000, a collection of Anglo-Saxon coins, including a previously unknown coin in the name of Albert, who can be identified with the Albert mentioned by post-Conquest sources as king in East Anglia circa AD 749. The discovery of a coin for a new Anglo-Saxon king is a major event. The Keeper also reported, as he does every meeting, a long list of modern coins and bank notes from various donors, including some 70 items from the donation boxes placed at strategic points around the Museum and from the 'pond' in the Greek and Roman Life Room which is strategically positioned right outside the entrance to the Coins and Medals Department.

The Keeper of Prints and Drawings reported two separate gifts of twentieth century material, while the Keeper of Oriental Antiquities reported gifts from four different donors, including herself, ranging in date from the fourth millennium BC (a Neolithic jade blade) to the twentieth century (six small boxes of Wuton and some examples of Chinese calligraphy). Western Asiatic Antiquities received two pottery vessels from Ur dating from the early second millennium BC, a bronze stamp seal from the fifth to fourth centuries BC and two Late Assyrian bronze fibuli, c. eighth century BC. Medieval and Later Antiquities were given a fifth century AD Byzantine copper alloy lamp by an American professor, while Ethnography were given two costumes from Burma in memory of the donor's husband, a decorated bark cloth and a plaited bag in the form of a fish from Tonga, from two separate donors, and a basket filled with artificial paper coins acquired in China between 1869 and 1884 from yet a fourth donor. The Keeper of Egyptian Antiquities reported a gift of £5,000 to cover the publication costs of a lecture from the people who had not only funded the lecture in the first place but also the refurbishment of two galleries which we opened last year and two which are currently in progress. The other report from Egyptian Antiquities was of a very different order financially, but equally exciting in its own way. It concerned a gift from the Petrie Museum here at University College of two First Dynasty inscribed sherds, the remainder of the inscription being on a sherd already in the BM collection which the two fragments join. The other cash gift reported this time was of £5,480.49 presented on behalf of Jehovah's Witnesses and their friends who visited the Museum during the period October 1991–March 1992. If you ever visit the Western Asiatic galleries on a Saturday you may well come across groups of Jehovah's Witnesses conducting study sessions in front of the sculpture or the showcases. The Museum takes no responsibility for the information they impart to each other, and intervenes in the study sessions only if they create an obstruction to the flow of visitor traffic generally. The periodic donations which we have now been receiving for some years seem to be a token of appreciation that when we do have to move them on we do so very tactfully.

Perhaps the most romantic gift this year was an Assyrian relief sculpture. It belongs with the reliefs from the great palace of Ashurbanipal which was excavated by W.K. Loftus at Nineveh and arrived in the BM in the 1850s. This object was known only from a drawing executed by Mr William Bouchter who accompanied Loftus to Mesopotamia. The drawing was published and republished many times and some references suggested that the dying lion

was in the BM, but there was no trace in our records. The donor turned out to be a Miss Bouchter who had always wondered how an Assyrian palace relief came to be hanging on the wall of the family home.

Of course we don't just sit and wait for things to come to us—we go out and look for them by means of excavation and field work. Although most countries have enacted legislation at some point this century restricting the excavation and export of antiquities from their territory, there are some parts of the world where archaeologists from the BM or from British universities are permitted and even welcome to dig. My colleagues have been involved this year in digs in France (in collaboration with Circonscription des Antiquités de Basse-Normandie), in Greece (working on the Hellenistic/Roman Theatre under the direction of Professor Waywell of Kings College London, and Professor Wilkes of this College and perhaps alongside one or two of you in the audience here today), in Egypt, Jordan, Pakistan, and the Sudan. The Trustees have also agreed to provide financial contributions e.g. to the Egypt Exploration Society's excavation in Egypt, to the British Archaeological expedition to a late Roman site in Northern Bulgaria (directed by Dr Poulter of the University of Nottingham, in collaboration with the Institute of Archaeology in Sofia) and to Dr Georgina Hermann's joint Anglo-Turkish-Turkmen project at Merv in Turkmenia. From some of these excavations we may expect to receive a share of the finds, in some cases perhaps only a token collection of study material, but in all cases the primary objective is to provide context for our existing collections.

The ethnographers have cast their nets wider still, with fieldwork and collecting trips to Morocco, Egypt, the Yemen, New Zealand, Tonga and Samoa and the Canadian Arctic.

Much of our work in this country is also carried out in collaboration with others. Various of my colleagues in the Prehistoric Department have this year directed the work of teams of varied specialists at an Iron Age settlement site in East Yorkshire, a Palaeolithic site in a disused clay-pit in Suffolk and at another Palaeolithic site in the Torbryan Valley in Devon. By this fourth and final season the team consisted of colleagues from the Natural History Museum, the National Museum of Wales, the universities of Lampeter, Bristol and Exeter and Oxford Archaeology Associates, as well as conservators, scientists and archaeologists from the British Museum. Right at the end of the previous season they had stumbled on the skeletal remains of an old man and a young woman, associated with a flint scraper and the leg bones of a horse, which was identified as a Bronze Age fissure burial, unexpected in that part of the country and hitherto a poorly understood monument type. Having been recorded with a three-dimensional digitiser provided by IBM, the burial can now be reconstructed on a computer for detailed spatial analysis. When I first read the report I hoped briefly that this meant the bones could be left to rest in peace, but I quickly realised that they would be subjected to a number of other forms of analysis first.

While the energies of our archaeologists are absorbed by this kind of scientific excavation, their attention is often drawn to the potential of the site by things that come out of the ground by accident or are found by amateurs with metal detectors. Every year some of our Purchase Grant has to be set aside for Treasure Trove finds. The origin of the law of Treasure Trove is lost in the mists of medieval history but the BM has a significant role in its application today.

Under English law coins and objects of precious metal, that is gold or silver (like those from Snettisham) found in circumstances which suggest they were concealed deliberately, belong to the Crown unless the owners or their descendants can be traced. Anyone finding a precious metal object or coin is therefore required by law to turn it in to the police and the find will become the subject of enquiry by a Coroner's Court. If it is declared Treasure Trove, it will be passed to the British Museum for detailed scholarly and technical examination. Single items or small groups of items are usually examined immediately by the curator with the greatest expertise in the appropriate field, but some of the larger hoards may involve many months' work. The Hoxne hoard which I mentioned earlier will occupy at least two people in PRBA and two in CM for several months while some of their work will only be possible after the material has been cleaned and treated in the Metal Conservation workshop. Once our report on the finds is completed it is passed to the Reviewing Committee on Treasure Trove, an independent body which advises the Treasury on the valuation of Treasure Trove. The finder is then awarded an *ex gratia* payment by the Treasury equivalent to the full market value of the object, (that is provided that he has complied with the law and declared the find immediately and not attempted to conceal it or sell the things on the black market) and the Museum is required to pay the Treasury the full market valuation of any of the objects which we wish to acquire. If the BM has no uncommitted funds at this juncture it either loses the find or is accused in the press of mean dealings. Finds which are not retained by museums are returned to the finders, who are free to dispose of them as they think fit. Even though the British Museum may have contributed months of time of its staff spent identifying and cleaning finds, neither the cost nor the added value is reimbursed.

Of course not all chance finds are precious metals—our most publicised acquisition of the 1980s, and star of a BBC QED programme seen on its first showing by some ten million people and focus of a BBC World Service programme which reached perhaps a hundred millon was undoubtedly Lindow Man, the body of an Iron Age man discovered in a bog in Cheshire, from which the peat was being dug for purposes connected with the commercial cultivation of mushrooms. The operator of a mechanical digger spotted what he recognised as a human leg in a shovel full of peat and had the presence of mind to call in the head of the local Archaeological Unit before digging out the area where the rest of the body lay. Local police were also called in in case the body should be that of a murder victim they had been looking for. Well, Lindow Man certainly met a violent end; he was garrotted, stabbed in the chest and hit on the head before being consigned to the bog, but samples tested at the Atomic Energy Establishment at Harwell pointed to a date around 500 BC, so the Cheshire police did not feel obliged to proceed with their enquiries. There was however immense scope for academic, archaeological enquiries, coordinated by Dr Brothwell, Reader in zooarchaeology here, at the Institute, which resulted in one of the archaeological best-sellers of the decade, *Bogman and the archaeology of people*. Lindow Man himself now occupies a position at the top of the main staircase where he daily entertains the crowd both in his rather pathetic, battered person and in holograph as reconstructed.

It is probably significant that one uses show-business vocabulary whenever the subject of temporary exhibitions comes up, for if there is one area of activity that has changed the face

of the BM in the last twenty years it is the temporary exhibition and the showmanship that has become an inseparable part of it. The *Dead Sea Scrolls* in 1966 was probably the first time that queues formed to see a foreign loan display at the BM but it was *Tutankhamen* in 1972 which put us firmly on the international exhibition scene. We now put on nine or ten shows a year, mostly drawn from our own collections but occasionally acting as hosts to a foreign loan exhibition.

The programme this year is fairly typical. At the moment we are showing three small exhibitions in the P&D gallery: a collection of eighteenth-century flower collages (given to the Museum by a great-great niece of the artist in 1897), a show of Samuel Palmer prints, and Europeans in Caricature. (The Department of Prints and Drawings thought it would be appropriate to celebrate the advent of the single market in the EC by displaying a selection of satirical prints demonstrating the prejudices and stereotypes that have determined Europeans views of each other for the past two centuries.) In the adjacent Oriental gallery, a colleague from the British Library has organised 'Britain's first view of China' illustrating another diplomatic theme, an event probably marginally less successful than Britain's current presidency of the European Community, viz. Lord Macartney's embassy to the Emperor of China in 1792. In the main temporary exhibition gallery we have just opened 'Howard Carter, before Tutankhamun' or as it is affectionately known 'The Not the Tut exhibition'. Though devoid of the gold mask or other famous relics of the boy-king, it is nevertheless a fascinating show and nobody has yet demanded their money back. It being Olympic year we used the space for another showing of the model of the site of the Ancient Olympic Games which was made for our summer exhibition in 1980 and gets trotted out to coincide with the summer games each Olympic year. The base was made in six parts so that it could be dismantled and reassembled with relative ease and thus act as the centrepiece of a travelling exhibition. It has toured this country and been set up in colleges, educational resource centres and libraries, as well as museums and has even made a brief appearance in Greece.

I have already mentioned the Skeleton at the Feast exhibition which many of you saw last month at the Museum of Mankind. You may also have been told about Paradise, which is to open there shortly. This sounds like an undue preoccupation with the after-life on the part of the Ethnography Department but in fact I believe the title refers to the birds of paradise which inhabit the New Guinea Highlands as do the Wahgi people who are the subject of the exhibition. The Ethnography Department also enjoyed a profile at Bloomsbury this summer with its exhibition of Mexican Painted Manuscripts before and after the Spanish Conquest. This brought together (almost) the entire corpus of Mexican codices in British collections.

Our summer exhibition in the adjacent Prints & Drawings gallery was Rembrandt drawings, held concurrently but running rather longer than the paintings exhibition at the National Gallery. The etchings on show at the National Gallery (which you could see without paying £5 to get into the main exhibition) were all on loan from the BM.

The Making of England opened the year in the main ground-floor Special Exhibition Gallery. It was a joint exhibition with the British Library, and a tribute to the retiring Director, Sir David Wilson, who spent a lot of his life thinking about the Anglo-Saxons both in the BM and at University College. At the opposite end of the building we were showing Nihonga,

traditional Japanese painting from the first four decades of this century. This was one of the BM's contributions to last year's Festival of Japan, as was the more spectacular exhibition which preceded it, Kamakura Sculpture. This was a unique opportunity to show some of Japan's greatest national treasures, assembled not only from museums but also from the temples which many of them had never previously left. It was welcomed not only by British and other western visitors to whom the sacred sculpture of medieval Japan was quite un-known, but also by our ever-growing numbers of Japanese visitors who had never been able to see many of the figures close to, in a good light or gathered together before. Both exhibitions were also part of an exchange loan programme linked to the fund-raising for the new Japanese galleries in which it was being shown.

I have mentioned the new Japanese Galleries several times so perhaps I should say a little about the project which played such a significant part in the life of the BM in the late 1980s.

The Galleries have been built as a new floor over the western half of the King Edward VII Building. The scheme includes a new students' room and air-conditioned storage for paint-ings, prints, books and lacquerware. The raised gallery at one end of the Konica Gallery, is named in recognition of a major donor, while at the opposite end of the complex is the Urasenke Gallery, named after the Urasenke Foundation, which funded it and built the authentic Tea House. A Teamaster from the Foundation conducts tea ceremonies once a month. Between lies the main gallery, funded by over 120 other major donors, led by the appeal's original supporter, Asahi Shimbun, one of Japan's largest national newspaper corpo-rations. Asahi Shimbun agreed to spearhead the BM's fund-raising in Japan on condition that the BM lent two major exhibitions from its collections to Japan. The first, 163 'Masterpieces of Japanese and Chinese Art' was shown to considerable acclaim at four venues: in Tokyo, Nara, Nagoya and Fukukoa in 1987. The second 257 'Treasures of the BM' was shown in Tokyo, Yamaguchi and Osaka and was seen by 1.3 million people in a few short weeks, breaking all sorts of records even in a country where attendance at cultural events can be predicted with confidence. A further condition of Asahi Shimbun's support for the Gallery project was our agreement to host a series of loan exhibitions from Japan, alternating with domestic shows from our own collections for the first five years of the life of the Gallery. Kamakura Sculpture was the second loan in, following 'Swords of the Samurai' in the previous winter. However, it was the success of 'Treasures of the BM' which put the decision-makers at Asahi Shimbun in the frame of mind to offer further and even more direct financial support to the BM by funding the Gallery of Amaravati Sculpture at the west end of the Hotung Gallery of Oriental Art. In return we shall be lending an exhibition of Buddhist sculpture including some of the finest pieces of Amaravati in 2 years' time.

Although we find ourselves occasionally as borrowers, as lenders we are in demand all the time. We lend complete exhibitions and contribute to dozens of others each year in various parts of the world, so that at any given moment literally hundreds of British Museum objects are on show elsewhere. Last year the BM lent 1,453 objects to 74 exhibitions in various parts of the UK and 1,317 objects to 55 exhibitions overseas.

Loans do of course involve risks, both of damage to objects, especially if they are fragile or have old repairs, and of loss.

It may well be the ever increasing volume of loan exhibition activity which is at least partly responsible for the rise in power and influence of the youngest branch of the museum profession—the Conservators. Conservation is now such a well-established discipline that it is hard to realise that it was not always so. The London *Illustrated Times* in 1862 was frankly critical:

> 'The Siamese monarchs,' it said, 'wishing to ruin their subordinates, presented them with live elephants. These were expensive enough, no doubt, but when they died there was an end. We, more enlightened, propose to bequeath dead elephants to our remotest posterity and meanwhile to pay for their lodging and keep... The BM is maintaining an unequal, expensive and useless struggle with that natural decay which is in the essence of things.'

I am sure the author of those sentiments would have been appalled to discover that the BM now has a whole department of some sixty trained Conservators dedicated to that unequal, expensive, but not, we would maintain, useless struggle against decay, as well as a Research Laboratory with some two dozen scientists working to make the struggle less unequal. And perhaps even more appalled to learn that the Institute turns out—how many is it—eight or ten more to do battle every year!

Needless to say, with collections as vast and as old as ours and an exhibition programme as intensive as the one I have described, there is more than enough conservation work to keep an even larger department hard at work for the indefinite future, even without the special problems of dispatching material to completely different climates, or patching it up when it comes back. These problems are sometimes met halfway by involving conservators as couriers for loans and of course also by involving them as *in situ* conservators at excavations.

Time is getting on and I think it is time that I draw breath and give you an opportunity to ask questions, but there is just one more point I should like to make first. Everywhere I go I am always hearing how much museums are changing, and how much they should change, as though change were a virtue in itself. I think you may have gathered that from the perspective of the British Museum the preference tends to be for continuity, and it is a break with tradition which requires justification. Of course I believe that we should exploit new technology to expand the scope of our traditional work on the collections, provided that new techniques do not become a end in themselves, that we should mount stimulating exhibitions which present new perceptions of the material in our care, but I should be very unhappy to see the British Museum turned into some sort of theme park, full of interactive devices and instant historical experiences. I have nothing against theme parks or heritage centres—some of them are excellent and informative as well as being enjoyable. To the extent that museums may be in competition with heritage centres and other visitor attractions, I believe that museums will succeed if they are good museums. I believed this ten years ago when three million people found their way to the British Museum despite the counter attractions springing up all over Europe. I believe it even more strongly in 1992 when it seems that six million visitors will come to the British Museum, and Euro Disney can't *give* its tickets away.

Abstract

The lecture provided a brief history of the British Museum and its role as parent of other national museums, galleries and the national library; an account of the growth and development of the collections in parallel to the history of the buildings provided to accommodate them, with particular reference to the joys and sorrows of a Grade I listed building; a brief survey of the present scope of the collections, and of current acquisition policy, with emphasis on creating as well as saving the heritage, particularly by excavation and by fieldwork; a tribute to the generosity of benefactors who give and bequeath their collections to the Museum. The work of the Conservation Department was familiar to many in the audience, likewise the extent of the exhibition programme, both temporary and long-term, which brings some six million people a year to the British Museum.

An Approach to the social dynamics
of northern Portugal's Late Prehistory

by SUSANA OLIVEIRA JORGE*

The period of time covered by this paper is very wide, ranging from the late third or early second millennium BC to the early first millennium BC, corresponding almost entirely to what has been named 'The Bronze Age'. On the Iberian Peninsula, especially in the north-west, this period encompassed communities that had reached various stages of social and economic development. With the progressive intensification of a mixed economy, these communities were experiencing the establishment of ever stronger and tighter mechanisms of social interaction, which varied both spatially and chronologically. Instead of trying to draw an evolutionary picture of these phenomena, which occurred over such a wide timescale, it will be more fruitful to examine only certain aspects of the development of the prehistoric communities that lived in the north of Portugal for *c.* 1300 years [1] (Fig. 1).

THE CHALCOLITHIC BACKGROUND

One of the most urgent topics of research at present appears to be that concerning the cultural antecedents that may have preceded and influenced the social transformations that occurred in some parts of the region at the turning point of the third and second millennia BC. These areas were marked by the presence of rich, individual interments and by a prestige trade based on metal objects, the latter linking the élite of communities at an early stage of social hierarchisation.

ECONOMIC INTENSIFICATION

In a recent paper (S. Jorge, 1986), it was suggested that the third millennium BC in northern Portugal was a decisive period both in an increasingly sedentary existence and in the occupation of the territories, which were, however, exploited unsystematically. In spite of cultural diversity and regional assymetries, and the difficulty of characterising certain socio-economic features of the late third or early second millennium BC communities, it is possible to perceive some points that these groups had in common. An apparently slow demographic increase seems to be correlated to:
a) an expansion into new regions, usually via good natural communication routes, e.g. the coast;

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b) a progressive investment in a mixed economy system;
c) a spread of inter-regional exchange and social relations, enhanced both by the procurement
   and the probable redistribution of non-local, or otherwise expensive raw materials.

All these factors converged in the late third millennium BC resulting in the emergence of communities with somewhat altered social structures, although still of an egalitarian nature. Research suggests that there was a possible trend towards socio-economic intensification which might have been partially responsible for these social transformations. Nevertheless, there is insufficient archaeological evidence to fully develop this hypothesis. Although a significant increase in production during the third and early second millennia BC can be indirectly observed in the settlement sites of the Chaves Aguiar region (S. Jorge, 1986), there are no data on the fauna and flora of the area so that it is impossible to characterise in detail the type of intensification that took place. The only clear environmental evidence recently published that is found with stratigraphic information consists of:
a) the Pastoria settlement, with scarce remains of ovicaprids and pigs found in both occupa-
   tional phases;
b) the Castelo de Aguiar settlement, with grains of bread and club wheat dating from the early
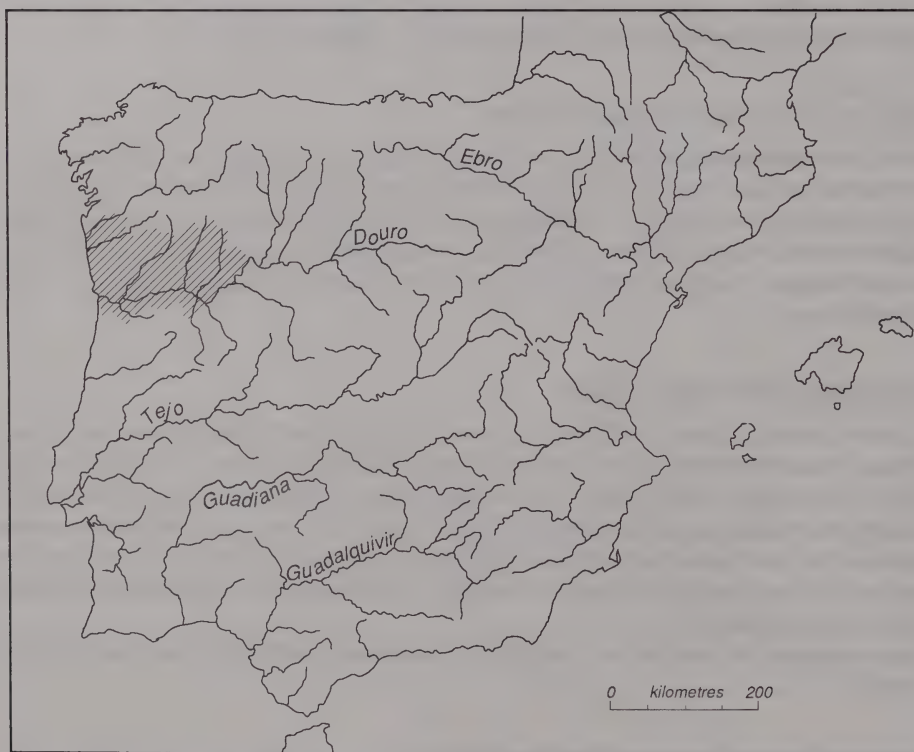   second millennium BC;

Fig. 1   The location of northern Portugal on the Iberian Peninsula.

c) the Buraco da Pala (Mirandela) settlement, with thousands of cereal grains found in an occupational period dated to the late third millennium BC (Sanches, 1987).

Unfortunately, it is not possible to confirm whether, during the late third millennium BC, northern Portugal underwent a set of socio-economic innovations that were part of what Sherratt (1981) has called, at a European level, 'The Secondary Products Revolution'. There appear to be similarities to several other Peninsular regions dating from the late Chalcolithic to the Bronze Age (Harrison and Moreno López, 1985); thus the social impact created by such a set of technological innovations in the populations that experienced the 'Secondary Products Revolution' cannot be ignored. However, the utilisation of animal secondary products such as the use of milk, cheese and wool, combined with the employment of animal traction, cannot so far be proven. Had the transformations mentioned by Sherratt (1981), Gilman (1981), Barker (1985) and others occurred, these might have drastically enhanced previously existing regional variations. These variations would then have become more noticeable because of the uneven spatial introduction of technological and economic innovations, both of them decisive driving forces behind social development.

Although there is insufficient direct archaeological evidence to demonstrate that the 'Secondary Products Revolution' occurred (at least during the third millennium BC) there is, nevertheless, considerable archaeological evidence testifying to changes in:

a) the location, dimensions and defensive features of certain settlements (Fig. 2);

b) the diversity of the material culture of settlements;

c) the monumentality of certain tombs, some of which are under excavation,[2] revealing a diverse range of grave goods.

The study of the cultural evolution in northern Portugal during the third millennium BC requires investigation in order to obtain clear evidence of the wide-ranging social developments that are known to have taken place in other Iberian regions during a transitional era of European history.

Fig. 2   A view of the Chalcolithic fortified settlement of Castelo Velho.

THE ROLE OF EARLY BEAKER POTTERY

Innovation in material culture can be perceived both in mortuary and domestic contexts in northern Portugal in the late third, as well as in most of the first half of the second millennium BC. In fact, the emergence of Beaker pottery in the 'Maritime Complex' can probably be dated from the late third millennium BC, where it is found in a sizeable number of tombs with mounds.

So far, however, it has only been found in a single settlement, that of Pastoria, a site whose decorated domestic ware is otherwise typical of other regional Chalcolithic sites (Fig. 3).

Although it is difficult to assess the chronological and stratigraphic importance of the earliest Beaker pottery, archaeologists have long been accustomed to relate those developments in a direct way to profound changes in the social sphere. It does not seem to have had great impact in the Trás-os-Montes area. Further west in Entre-Douro-e-Minho, the presence of such pottery is notable from the beginning, although disassociated from other typical elements of the Beaker phenomenon, in what were probably varied cultural contexts whose diversity it could prove useful to analyse further. It is necessary to ask if one type of pottery would have played only one overall symbolic role, or whether it should be seen as having been of varied significance according to region. It is postulated that, for the first time, a part of the community would have possessed a decorated artefact type with inter-regional prestige, and its presence presumes the existence of a system of relationships between communities that had already formed.

In certain communities, Beaker pottery might also have signalled the rise of an élite, although of varying characteristics. This élite was strengthened during the third millennium BC as the agricultural system became more intensified. At the same time, trade routes for rare or non-local raw materials, such as copper, flint and textiles (the latter only in the final part of the millennium), as well as other materials that have left no visible archaeological trace, were slowly coming under the control of certain groups.

Nevertheless, during this first phase, when Beaker pottery still appears to have been uncommon, the social and economic structures of the recipient communities may not yet have undergone profound transformation. As clay could be procured locally without great cost, its production did not interfere with the established circuits for the acquisition and distribution of rare or otherwise expensive commodities. This particular aspect seems to be of great importance for the understanding of the finding of the earliest Beaker pottery in more culturally 'archaic' contexts, such as burial mounds or the Chalcolithic settlement of Pastoria. Beaker pottery appears therefore to have been assimilated without disruption (at least disruption that is visible to the archaeologist) by some local communities whose élite wished to acquire a new power status.

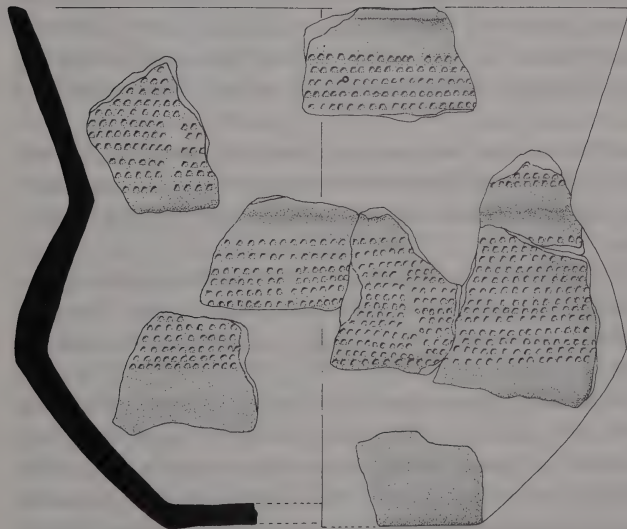## THE EARLY BRONZE AGE

During the first half of the second millennium BC, the social landscape of northern Portugal appears to have undergone noticeable change, with an increased number of funerary contexts, special deposits (hoards?) and stray finds of varied character.
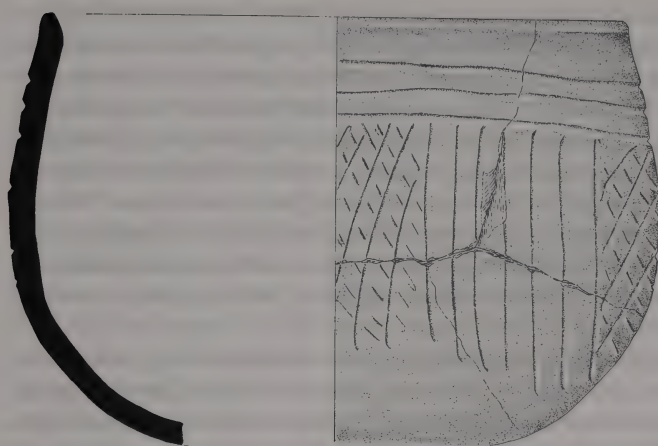
0 ⌐————————⌐ 4 cm   3a. Penha Type pottery.

3b. Beaker pottery.

Fig. 3   Chalcolithic pottery from northern Portugal.

Without attempting an exhaustive study, some examples of known contexts are discussed. The passage grave with covering mound of Madorras (Sabrosa) yielded Beaker pottery of the Maritime, Palmela and Ciempozuelos groups, as did the passage grave or cist-like chamber with covering mound of Chã do Carvalhal 1 (Baião). In this latter mound two tanged daggers and five copper Palmela points probably dating from construction phase of the monument were also found *in situ*. In the tombs with covering mounds at Vilar (Vila do Conde) and Chã de Arefe (Barcelos), stone wrist guards and copper Palmela points were found. The flat interment (individual cist) of Quinta da Agua Branca (V. N. de Cerveira) yielded a large tanged dagger of arsenical copper, gold spirals and rings plus a gold decorated diadem. In another flat tomb (cist?) in S. Bento de Balugães (Barcelos), three Palmela points of arsenical copper and a gold collar were found.

It is interesting to note the coexistence of two types of interment at the beginning of the second millennium BC: those of a 'classical' Beaker character (e.g. Chã de Carvalhal 1), and those of the Montelavar type (e.g. Quinta da Agua Branca, S. Bento de Balugães or even Chã de Arefe). These inhumation types contained at least partially similar grave goods but in different combinations. Nevertheless, the distribution of the Montelavar group is largely coastal, in contrast to the mainly inland location of late Beaker vessel interments. Only the Tapado da Caldeira (Baião) settlement has, so far, yielded Beaker pottery where it is possible to recognise decorative themes common both to the Ciempozuelos complex and to some regional styles.

It is necessary to refer to the existence of another type of mortuary interment. The finds from these tombs appear to be poor (consisting mainly of pottery) but they also reveal the native cultural background. The monuments with mound and cist-like chambers of Outeiro de Gregos 1 and Meninas do Crasto 4 (Baião), Antela de Portelagem (Esposende), Lomba de Coimbró (Montalegre) or Portela de Gorgurão (Boticas) can be included in this particular phase (S. Jorge, 1986).

Finally, special deposits (hoards?) and stray finds made of arsenical copper and gold also occur throughout the region under study showing that these were part of a wider network of prestige metal objects, a network whose boundaries extended well beyond the north-western Iberian region itself (ibid, 1986).

Unfortunately for this period, most of the evidence consists of stray finds, supplying very faulty and uneven information for the reconstruction of existing social landscapes. Nevertheless, the data illustrate a remarkable variety of burial types, such as individual inhumations, and the emergence of a new range of prestige object types.

Individual inhumations are now recorded for the first time. They were small in size, and sometimes totally unobtrusive within the landscape. If the earlier and larger megalithic tombs relied on the participation of the entire community for their construction, the new and smaller tombs would have had no need for such a large work-force. A new type of relationship between the community and the power-holding élite may therefore have become established.

There appears to have been a substitution of certain artefact types, such as vessels or idols, by a new range of prestige objects, such as weapons and jewellery, that reflected more personalised and individual power relationships. This change in grave goods reflected not

only the emergence of a new type of male leadership, but more importantly denoted a redirection of the ideology of power. This new power-base suggested that there must have been a new pattern of inter-regional networks that now encompassed the whole of the Atlantic coast.

According to Shennan (1982, 1986) power, now measured by the ownership of certain patterned artefacts which had acquired an intrinsic value, was being used to legitimize social status. Moreover, the metal ores used for these new prestige objects could only be found in specific areas, creating the need for a network of alliances between sometimes distant communities but which shared the same level of social complexity. These intra-communal relationships may partially explain the expansion of the Beaker phenomenon as part of Renfrew's concept of Peer Polity Interaction (1982, 1986). The core of such a concept is that a new type of integration could come about between communities as a result of the pervasive character of the network of social relationships. In order that some prestige items might circulate freely among communities at a similar social level, the existence of a network of reciprocal procurement would have been necessary in areas that were initially cut off from a direct network of interaction (Shennan, 1982). This would therefore lead to the inclusion of communities at a lower level of social complexity in the network. These groups would act as 'middle-men' in a process which they did not initiate, but which would slowly absorb them. This particular perspective could explain the observed variability of funerary contexts that were more or less strongly 'influenced' by the Beaker phenomenon in northern Portugal. The contrast of tombs with covering mounds and relatively plain artefacts to individual inhumation cists with considerably diversified and wealthy artefacts probably reflected the heterogenous social realities which were briefly part of the same network of long-distance contacts.

It is interesting therefore in studying the particular reasons for the adoption of Beaker elements within communities, to ask why such a phenomenon left no visible trace in several regions of northern Portugal. What, for instance, might explain the observed lack of Beaker elements in Eastern Trás-os-Montes where, however, for example, some exceptional copper objects such as the Carrapatas type halberds circulated?

If the presence of metal artefacts of the Montevalar Group in the east and the presence of Ciempozuelos pottery in the west testify to possible relationships between the Atlantic coast communities and those of the inner northern Meseta, it must be due to the participation in the exchange of such prestige artefacts between the populations. The contact of less complex societies with artefacts and ideas of more sophisticated societies may have led to emulation. The specific cultural evolution of the Trás-os-Montes population, for example, might have been influenced in some way by contact with more complex societies, though as yet this is not perceptible in the archaeological record.

Finally, it is necessary to note the difficulty in examining social hierarchisation from archaeological information alone, and in the absence of known settlements, the archaeologist usually uses funerary data. In the case of the area under study, these data consist of truncated sets of funerary artefacts and tombs found in isolation. The interpretation of such data can be problematic as different concepts of hierarchy could have led to the adoption of different funerary rites. It can be subjective, therefore, to classify *a priori*, for instance, a society as

'less hierarchical' because of the construction of individual inhumation cists, containing only ceramic vessels and not metal objects. As Peebles and Kus have pointed out, social hierarchisation can only be measured through a set of interacting phenomena observed in the archaeological record (1977). These phenomena could be related to the appearance of complex levels of tumuli differentiation, the presence of autonomous subsistence activities and the existence of specialised craftsmanship, even if not full-time. Following these criteria, the data currently available concerning the functioning of early second millennium BC societies in northern Portugal are very scarce. Unfortunately, a great deal of research remains to be done on second as well as third millennium BC societies, necessitating the design and implementation of regional projects to define settlement patterns in well-defined areas. It is possible to reach an interpretation of more intense inter-regional contacts, on which the internal trajectories of certain populations depended, only through a simultaneous analysis of single settlements and their place within a wider system of inter-community cooperation. Therefore a theoretical approach, taking into account the varied nature of the archaeological record, is needed.

THE MIDDLE BRONZE AGE

In northern Portugal the period from the seventeenth to the fourteenth century of the second millennium BC is particularly in need of data about cultural evolution. This period is conventionally known in European terminology as the 'Middle Bronze Age'. In fact, the specificity of this phase is very difficult to determine. Recent discussion of the terminology used for the Iberian Bronze Age (Ruiz-Gálvez Priego, 1984b; Fernández Manzano, 1985) partially derived from arguments put forward by English authors, especially concerning the discontinuities and continuities in relation to previous centuries, which has led to debate on the evidence identified in this area.

METALS AND METAL ARTEFACTS

Research on metals and metal artefacts dealt exclusively with the identification of stray finds. Although some of these artefacts have been subjected to thorough analysis, there is usually no information either about their metallographic composition or processes of procurement, nor have they undergone any thorough typological analysis. In addition, some morphological patterns appear to have remained unchanged from the previous phase—for example, flat axes made of arsenical copper. This typological immobility has led to obvious difficulties regarding the dating of such artefacts. Several authors have insisted therefore on a certain 'isolation' of northern Portugal (and of the north-western area in general) during this period, in contrast to other Peninsular areas, especially the Atlantic coast (Briard, 1965, 1976; Ruiz-Gálvez Priego, 1987; Coffyn, 1985). Coffyn has tried to explain such a possible retraction in Atlantic contacts by the appearance, during the so-called 'Middle Bronze Age', of new circuits of procurement for arsenical copper, traditionally linked to the Iberian peninsula, but now of Continental origin. This new circuit would then have supplied the Britanny and Medoc regions and west central Europe.

Nevertheless, the data available, although scarce, might indicate a slightly different perspective from that which is usually argued. Indeed, towards the end of the first half or early second half of the second millennium BC, (following the Ruiz-Gálvez Preigo chronology), the first flanged axes can be found, some still of arsenical copper but others already made of bronze. A sword of arsenical copper (often seen as proto-Argaric) from S. Bartolomeu do Mar (Esposende) which Almagro-Gorbea (1976) approximates to the examples from Brittany of the Treboul-St Brandan type, could in fact date from *c.* 1600/1500 BC. Later on, during the Middle Bronze Age I, the first bronze axes of 'Bujões-Barcelos' type can be identified. Ruiz-Gálvez Priego includes in this phase the bronze spearheads of Lama Chã (Montalegre), probably of local production. This particular type of spearhead seems to have no parallel in known contemporaneous contexts. Nevertheless, Coffyn has dated such objects to the Late Bronze Age III. In the immediately preceding phase of *c.* 1200 BC (Middle Bronze Age II), apart from the Bujões-Barcelos axes, bronze axes and non-looped palstaves of Atlantic inspiration can be identified.

Thus, during the 400 to 500 years preceding the development of the so-called Late Bronze Age, not only was there an important technological innovation, that of bronze alloy, but also, either directly or indirectly, Atlantic influences were still present (although the archaeological evidence for this becomes weaker). These Atlantic influences are clearly visible both in the typology of the Corvilho, S. Tirso (Bignan type) and the Arnozela (Fafe) bracelets.

During this phase, however, both tombs that can be unequivocally dated to this period, and settlements are notably absent. One small cemetery with probable single inhumation graves, each one containing a ceramic vessel, is dated towards the end of this period and nearly into the Late Bronze Age. This is the Tapado da Caldeira (Baião) necropolis. Other graves with a similar morphology could eventually be placed in the same period or even later (S. Jorge, 1988). The absence of wealthy funerary artefacts with prestige metal objects, however, in the early second millennium BC should be emphasised.

SOCIAL CHANGE

In the absence of tombs and settlements, the survival of certain types of metal artefact of the so-called Early Bronze Age, and dispersed stray finds have led several authors to question the cultural character of this period and to assimilate it to the previous one (Ruiz-Gálvez Priego, 1984a, 1984b; Coffyn, 1985). It may be too early to use negative evidence to draw any kind of conclusions about the character of this phase, because of a lack of systematic study. In order culturally to characterise northern Portugal during this period, it must be considered as a whole. Nevertheless, some features should be highlighted. The appearance of bronze artefacts, for example, is indicative of increasing social development and points to technological innovation, but also reflects the probable control by some populations of routes both of procurement and distribution of both copper and tin. This implies that an inter-regional network of contacts substantially more complex than that of the early second millennium BC now existed. Nevertheless, in spite of some Atlantic influences which could account for these interactions, communities should not be assessed on the same cultural patterns as those from

the beginning of the millennium. The absence of tombs may not be a reflection of deficient research. In a similar way to the Middle Bronze Age in Brittany (Briard, 1984), a qualitative change in funerary contexts might have been true of the north-east. This change would have come about through a devaluation of the wealth of grave goods, due to an alteration in funerary symbolism. Symbols of power would have been transferred from the mortuary sphere to special deposits or hoards of metal objects displaying diverse meanings. Known stray finds could have belonged to these special deposits.

Such a hypothesis would bring the populations of this period closer to those of the Late Bronze Age, both in social and symbolic terms, rather than to those of the early second millennium BC. Therefore, the keeping of certain metal 'models' of a former tradition might not correspond in itself to the permanence of previous patterns of behaviour but only to a stylistic conservatism whose causes need to be studied.

The period traditionally known as the Middle Bronze Age, would correspond not to a period of stagnation, but instead to a period of slow change. This period would include some artefactual archaisms that were inserted into a social reality which prefigured the Late Bronze Age.

Therefore, if there are any reasons to criticise the traditional tripartite division of the north-western Bronze Age they aim at around *c.* 1600 BC. This would require the acceptance, however, of the suggestions discussed above, which are not possible to prove at present. Despite the fact that in certain structural aspects, the so-called Middle Bronze Age is related to the Late Bronze Age, certain evidence points to this period as being in some way unique.

THE LATE BRONZE AGE

Only in the last few years has the north-west of the Peninsula been the object of synthetic study on metal production during the Late Bronze Age, placing it within a wider picture of Atlantic relations which were increased from *c.* 1250 BC and maintained until the eighth or seventh centuries BC (Ruiz-Gálvez Priego, 1987; Coffyn, 1985; Almagro-Gorbea, 1986). Nevertheless, so far, studies on metal objects have been concerned either with:
a) typological classification;
b) the metallographic analyses and study of the production processes, although only of some of the object types;
c) or a general comparison with other object types in order to establish chronologies and diffusion routes of raw materials, artefact types and the study of morphological patterns.

For some time, discussion of metal artefact types in northern Portugal has usually been focused on stray finds. Unfortunately, complete hoards have not yet been found that could, at least, provide minimal information on the mechanisms of long distance circulation of such products, if not on the social mechanisms that led to them being hidden intentionally.

Recent excavations on settlements belonging to this general phase have increased the information available on aspects related to settlement strategy, forms of land use, intra-settlement organisation and the diversity of the material culture, the latter including some rare metal objects. Due to the incipient state of research at present, such contributions have helped

enormously to broaden the range of questions that can be asked about northern Portugal in the Late Bronze Age.

Based on currently available data, there appear to be at least two stages of cultural development. The first ranges from between *c.* 1250 BC to 1000/900 BC. The second occupies the period from the latter date to *c.* 700/600 BC. These chronological divisions will now be justified.

THE LATE BRONZE AGE: THE FIRST PHASE

During the initial phase of the Late Bronze Age, it seems possible to observe the incipient production and circulation of bronze artefacts, such as solid palstaves and axes. Examples of jewellery of the 'Villena-Estremoz' type can still be included in this phase, although swords are absent. Special deposits in the form of hoards are not known, however; some stray finds may have been part of such a set of specially stored objects. The general lack of data might correspond to the accidental nature of retrieval of these objects.

If there is nothing remarkable about the metal production at this time, data concerning settlements with large pits which recent study has identified as underground storage structures, such as at the Bouça do Frade settlement (Fig. 4), clearly indicate a remarkable transformation of the subsistence systems. These structures would have been able to store enormous quantities of seeds and basic foods, pointing unequivocally to a more intensive farming economy. This economic development would only have been possible through the use of sophisticated farming technology and the probable use of animal traction. Such an intensification of the economy would have facilitated the emergence of mechanisms of self-sufficient subsistence at a settlement, or even at a regional level, a necessary condition for the development of strongly hierarchical communities. Considering what is known of other contemporary contexts, one inference is that these settlements would eventually have been incorporated into organised settlement systems. Future research should direct particular attention to culturally and geographically homogenous areas in order to avoid generalisations. Only then can geographical, but also hierarchical settlement patterning, as well as the relationships of communities with surrounding environment be examined.

The Tapado da Caldeira cemetery, located only a few metres away from the Bouça do Frade site, probably coexisted during the first phase of the settlement. This cemetery has given an important clue to the social organisation of that population through the presence of an unusually small, possibly child-size grave, suggesting the possibility of inherited social status. It is not surprising that this social shift occurred during a period of economic intensification when ownership of land would have been fundamental for the maintenance of élite stability. Moreover, if rigid inheritance mechanisms were not secured, land ownership and transmission could have generated strong competition within social groups. In the context of societies that produced and distributed metal artefacts in northern Portugal, with important sources of tin and copper, élite power and hierarchical relations would not have depended exclusively on the control of the production of subsistence goods, but would also have depended on the manipulation of the networks of metal circulation and exchange. These
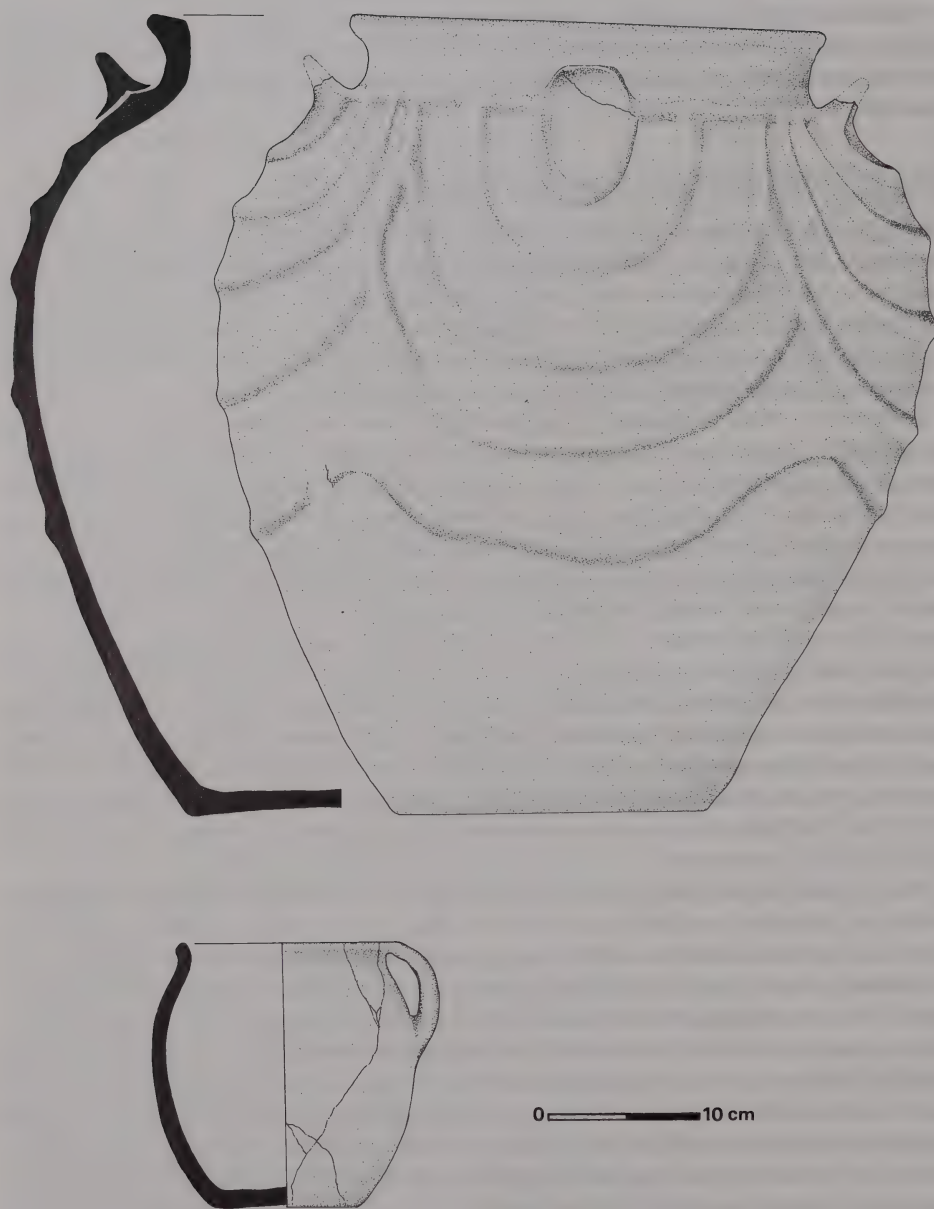
Fig. 4   Two vases from the Late Bronze Age settlement of Bouça do Frade (Baião).

communities would have had to be integrated into a wider network of inter-regional alliances whose features, so far, are unknown.

In the initial phase of the Late Bronze Age, Cogotas I type pottery is found in certain settlements and in one grave in northern Portugal. The spread of such pottery towards the west as far as the Atlantic coast is difficult to explain, although this may have been part of a wider context of procurement of metal ores, possibly tin, by some of the northern Meseta populations. In the Meseta, however, copper had been exploited since the Chalcolithic as had tin, although only from the beginning of the Late Bronze Age (Fernández Manzano, 1986). Indeed, despite the longstanding metallurgic autonomy of the region (Delibes de Castro and Fernández-Miranda, 1981, 1983), it is probable that the dynamism of local workshops was only imposed completely after the early tenth century BC. It is precisely before this chronological milestone, however, that most of the 'classic' Cogotas I ceramic types seem to have occurred in northern Portugal (S. Jorge, 1986). This need not contradict the hypothesis that pottery can demonstrate the procurement of metal ores, however, as this pottery may have spread via the procurement of other, non-metal raw materials and of prestige objects from the Atlantic area. It might then have spread amongst the local populations as an item of lesser social value. Nevertheless, despite its presence there are no signs of strong competition between the region's communities, as might be expected.

In conclusion, in the first stage of the Late Bronze Age, although data are scarce, they point to apparently non-fortified settlements with an intensive mixed farming economy and an apparently limited circulation of metal artefacts. Important evidence of social competition is not visible, at least in the archaeological record.

THE LATE BRONZE AGE: THE SECOND PHASE

During the second phase (which can be subdivided) changes at several levels took place. Firstly, a larger quantity and a progressive diversity of circulating metal artefacts can be noted, such as palstaves, socketed axes, socketed spearheads, daggers and one probable sword. In addition, both domestic and prestige objects were found, such as sickles, knives, chisels, fibulae, helmets, cauldrons, metal hooks and spits. Most of these artefacts could have been of local origin although made according to foreign designs, mostly of Atlantic origin. No sealed deposits are known, but most of these stray objects may have belonged to such intentional deposits. Only a few of these have been found in settlements. One of the main research topics for this period, as for the entire Middle or Late Bronze Age, should be to address the rarity of metal artefacts in domestic contexts and the varied meanings that the special deposits may have assumed, whether of 'utilitarian' or 'votive' character. Since an object might have assumed different functional and symbolic values according to the different communities that either produced or received them, it is necessary to define criteria for the identification of the meaning of an artefact. Objects can be considered either as commodities of simple exchange which were temporarily stored in a certain place, or as prestige items for élite consumption (Bradley, 1985b).

With the progressive utilisation of local workshops, these workshops themselves may have

diversified and increased their production, especially during the eighth century BC. They would also have intensified their relations with both the traditional Atlantic production centres and with new workshops located in the Mediterranean, for example, that in Sardinia (Ruiz-Gálvez Priego, 1986, 1987). The need to exchange products in new areas of the Atlantic market must have provoked strong competition between the different Iberian production centres at an inter-regional level. During this phase of greater metallurgical activity it would be useful to determine the role played by the different regions in this wider network of exchange. If some areas were fundamentally differentiated by being suppliers of raw materials, such as the north-west and south-west regions and Beira Alta, while others were 'in-between' areas, such as the Portuguese Estremadura region, then the emergence of important inequalities in the circulation control of products and associated power assymetries must have been unavoidable among the competing socio-political units. Furthermore, this scenario on a Peninsular scale must have had repercussions at a regional level.

The emergence of fortified settlements at a higher altitude, independently of the survival of other types of site, combining natural or human defences in northern Portugal in the early first millennium BC, should be considered within this context of social instability. Settlement polymorphism in a clearly defined region, such as the Ovil river valley, Baião, for example, suggests the probable partial coexistence of open settlements with large pits on the one hand, and naturally defended settlements on the other (S. Jorge, 1988). This reinforces the hypothesis of a hierarchical settlement pattern, but also points to the possible existence of central places. These settlements would not only have appropriated the production and circulation of metal artefacts at a local level, but would also have acted as a focal point for the redistribution system for subsistence products.

Although the dynamics of these local communities were part of a wider process of social transformation within an Atlantic exchange network, the emergence of this new type of settlement at the beginning of the first millennium BC does not need to be justified by a migration hypothesis acting externally to these dynamics. Indeed, the Urnfield phenomenon only reached the northern Meseta in the late eighth century (Ruiz Zapatero, 1985; Almagro-Gorbea, 1986). Moreover, hypothetical influences, even indirect ones, of this cultural 'movement' in the north-west can only be observed after the seventh century BC, i.e. at the beginning of the Iron Age. However, as Ruiz-Gálvez Priego has also argued (1987), even during this period it is difficult to see whether the Urnfield phenomenon might have been significant, or whether it might have determined in some way the trajectory of protohistoric communities in this region.

THE IRON AGE TRANSITION: SOCIAL REORIENTATION

Finally it is necessary to examine some factors that accelerated the decline of the network of Late Bronze Age relations with the Atlantic in northern Portugal. As Phoenicians were in control of the Gibraltar Straits from the late eighth or early seventh century BC, it may have proved difficult for the northern Peninsular communities to have had access to the copper sources in the southern Peninsula. Since the Atlantic contacts were largely subsidiary to the

production centres of copper in the south-west and tin in the north-west Peninsula, Phoenician interference would not only have exacerbated the decline of long-standing cultural relations between the Peninsula, western-central France and the British Isles, but would also have influenced, in the medium term, a reorientation of the socio-economic strategies of the Iberian communities. The latter were now affected by the impossibility of assuring a free circulation of products in the traditional corridor between Galiza and Andalusia. A 'residual bronze metallurgy' (Almagro-Gorbea, 1986) may have endured in the north-east due to the survival of tin production centres. Nothing is known, however, about the social structures that might have permitted the survival of these centres in the face of a progressive breakdown of the previous system of inter-regional relationships.

How was this cultural reorientation expressed? What forms did it take in the archaeological record? Some elements of oriental inspiration, probably dated to the seventh century BC,[3] suggest that during this phase the axis of inter-community relations no longer lay only in one direction and that the nature of those relations might have changed. As tin ores from the north-west came under the control of the Phoenicians or their contacts from the late seventh century BC, this would have undermined the socio-economic system that had characterised the previous centuries.

In her work on the protohistory and Romanisation of the Cávado valley, Manuela Martins (1987) has discussed the difficulty in identifying the settlement pattern of the communities which made the transition from the Late Bronze Age to the Iron Age. Nevertheless, communities occupying fortified settlements, some of which had been occupied since the Late Bronze Age, start to present a markedly different material culture, with domestic ware displaying distinctive new features. In addition, it has proved difficult to explain the origin of the 'Castreja Culture' in fortified settlements from the later phase of the Late Bronze Age (Almeida, 1974; Silva, 1986; Martins, 1987).

Several Late Bronze Age settlements, some of them with natural or man-made defensive systems, were located in areas that had not previously been occupied—such as, for example, all the sites known so far in the Ovil valley, Baião. Some fortified Late Bronze Age settlements were reoccupied in the following period, but in different areas of the same hill or spur, with no overlapping of stratigraphy. New occupation reveals a reorientation of locational strategy, such as the S. Julião and Barbudo settlements. Some of these sites appear to have been occupied continuously, such as Coto da Peña, but architectural changes in the domestic and defensive features may have taken place suggesting general changes that are as yet insufficiently understood. In those cases, such as at S. Julião and Barbudo, the transitional material culture of the seventh or sixth centuries BC is considerably changed compared to that of the Late Bronze Age. Although alterations in material culture need not always express any deep cultural transformation (Hodder, 1982a, 1982c), there appears at least to have been some kind of functional change.

The emergence of Late Bronze Age fortified settlements seems to have been rooted in a socio-economic system which started to collapse in the late seventh century BC. That system stimulated competition between local groups and 'socio-political regional units' and this was channelled through a complex network of long-distance exchanges of Atlantic character.

Given these arguments, if the reoccurrence of defence systems in some of the settlements occupied since the Late Bronze Age was regarded as proof of cultural continuity, this would devalue the particular social context that might have permitted the construction of such systems in each case.

The grounds for asserting the origin of the so-called 'Castreja Culture' in the Late Bronze Age are flimsy, and it might prove more useful to try to understand the causes that might have simulated the specific fortification of Iron Age settlements in each region. Such an approach, that would necessitate the implementation of regional studies,[4] would be important not only for the characterisation of the first protohistoric populations, but could also provide interesting data on continuity from the Late Bronze Age into later periods. This information might permit the establishment of eventual links between the Late Bronze Age and the Iron Age which traditionally are divided.

STATUE-MENHIRS

In concluding these reflections on the Late Bronze Age in northern Portugal, attention should be drawn to the presence, not yet properly assessed, of a family of statue-menhirs. This 'family' is heterogenous in its attributes and probably in its chronology. Known examples are from Chaves, Faiões, (Chaves) Ermida, (Ponte da Barca) and from an unknown location in northern Portugal (kept in Porto). As has been stated before, these statue-menhirs display an anthropomorphic character given by a sinuous contour that accentuates the shape of the head, arms and/or waist (S. Jorge, 1985). Apart from the example at Ermida, whose chronology is difficult to pinpoint, the remaining examples display strong morphological similarities, such as sheathed weapons, and a sort of long vest, an adornment of rectangular shape at the back, with a slight increase in width at its bottom end in the Faiões and Chaves examples. The latter feature is also present in the phallic statue-menhir (?) found at Bouça, Mirandela, (Sanches and V. Jorge, 1987), although this particular example does not display any kind of weapon and its date is difficult to determine. The Porto example shows a helmet, which might indicate a wider chronology as it can be dated to a later period than the Chaves and Faiões examples. The particular form of the heads and the general conception of the Faiões, Porto and also Ermida statues suggest formal similarities to the Corsican statue of the middle second millennium BC. Independently of this, the following features should be highlighted. Firstly, three of the male characters represented are bearing weapons, and all of the examples reveal a conception of sculpture at a proto-statue level. This may have led to the well-known production of sculpture in the 'Castreja Culture' of northern Portugal.

There may have been an eventual linking of some of the statue-menhirs, at least those bearing weapons, with the emergence of communities led by strongly competitive élites during the Bronze Age, especially in the Late Bronze Age. These élites would have controlled both the production and the long-distance circulation of commodities. Apart from the 'Porto' statue-menhir, those that came from Chaves were found in an area close to an important tin centre. In the same way as in the south-west, where the stelae were an expression of a very hierarchical society, a similar social process could have found equivalent expression in the

creation of these symbols of individual power relating to local chiefs, whether deified or not, in northern Portugal.

CONCLUSION

Apart from sparse influxes from northern Meseta, northern Portugal was the subject of a pendulum-like alternation between Atlantic and Mediterranean 'influences' throughout Late Prehistory. These 'influences' were manifested at the level of material culture. Mediterranean 'influences' were stronger during the third millennium BC, although they were also present in the Late Bronze Age. Atlantic 'influences', although always present, left a more visible mark during the Early and Late Bronze Ages. Nevertheless, the stylistic similarities of artefacts at several points over this time-scale, must surely indicate forms of social interaction whose nature changed according to time, region and cultural context.

During the Late Bronze Age, similarities among certain Atlantic coast artefacts, such as metal objects and non-domestic wares can be explained as important inter-regional transactions. The forms of contact that took place, however, between the earlier Chalcolithic and Early Bronze Age societies are much more difficult to characterise, due to the profound interaction of social and economic structures that took place within an incipient social hierarchy. Therefore, it is much more difficult to determine the causes which might have led to the occurrence of certain stylistic patterns of southern origin in the domestic ware of third millennium BC settlements, than to explain the typological similarity of non-domestic pottery of the Late Bronze Age settlements on the Iberian Atlantic coast.

It is difficult to identify 'cultural influences', because of the need to define the nature of the relation between material culture and the respective social organisation, i.e. the connection between artefactual similarity and possible social interaction (Schiffer, 1976; Wobst, 1977; Hodder, 1979, 1982b; Plog, 1978, 1980).

Discussion of these questions, debated by several scholars from the United States and Britain, makes it possible to examine some aspects of research on Late Prehistory in this region. Irrespective of time, each social context presents its own functioning logic. There are no precise rules, therefore, that command the stylistic variability of an artefact. Although different communities, while culturally distinct, can manipulate similar artefact types, one cultural group can manifest a set of diversified objects within its own area.

It might prove useful not to confuse elements belonging to the domestic sphere, which normally transmit the visible identity of a group, with elements of material culture that are rare. This distinction is fundamental when comparing types of artefacts from different regions. The similarities observed therefore during the Chalcolithic between the ceramics of northern Portugal and those of southern Iberia must be seen as being part of the domestic sphere, whilst those found in the Late Bronze Age between metal objects and burnished ceramics from several points along the Atlantic region are related to the world of the exotica. The mechanisms of interaction responsible for such similarities are necessarily diverse, independent of time and the cultural systems within which they took place.

Stylistic variability amongst domestic artefacts can be interpreted in certain well-defined

circumstances, e.g. under economic pressure and inter-community competition, as a communal answer to existing social instability (Hodder, 1982c). In these cases, artefacts, particularly domestic wares, can be used as a device of social integration and cohesion. In similar circumstances, however, other types of rare or prestigious patterned artefacts may have circulated between culturally varied communities, maintaining the fundamental role of relieving stress, such as latent conflict, and therefore preserving the intra- and inter-community balance. Thus, the circulation of certain well-patterned objects in Late Prehistory such as Beaker or Baiões type pottery, tanged daggers, Palmela points, copper or bronze axes, etc., could have increased in the same proportion as the level of competition among the communities, at least in certain cases. The geographical delimitation of the stylistic territories of certain object types therefore does not correspond in a direct way to culturally homogenous areas, but only to regions through which certain socio-technical items circulated.

In these circumstances, the existence of Atlantic, Mediterranean or other 'influences' in northern Portugal during Later Prehistory will only make full sense when, apart from the observation of the typological affinities of material culture, it is possible to determine the specificity of the social structures that permitted the occurrence of such similarities.

Notes

1. These areas consist of the Area of Minho, Douro Litoral, Trás-os-Montes and Alto Douro.
2. Recent studies (not yet published) carried out by E. Jorge da Silva of Portucalense University on funerary monuments on the Minho coast have contributed to the reassessment of the traditional picture of a certain 'poverty' of northern Portugal's megalithic grave goods.
3. Apart from the the famous Baião treasure, there is in existence a vase from Pedroso, Celorico de Basto, with grafitti. According to Almagro-Gorbea, this seems to be a local imitation of oriental writing.
4. Such studies in the Entre-Douro-e-Minho region include the model approach carried out by M. Martins in the middle valley of the River Cávado, and the present undertaking by archaeologists based at the Archaeology Institute of the Faculty of Letters, Porto University.

Abstract

For the past ten years, northern Portugal has been the subject of systematic research by prehistorians, and from this have resulted regional studies, some of which are still being carried out. These studies not only concentrate on the need to obtain a chronological sequence of social development, but also on identifying patterns in spatial settlement.

Some general problems are apparent from the available information. In this paper, three main themes are addressed. Firstly, the changes that occurred during the third millennium BC —the development of a mixed farming economy, the occupation of new territories, an increasing sedentary existence—and mechanisms of social interaction are explored. Secondly, the Beaker phenomenon in northern Portugal is examined, with possible explanations for its significance in the period between the third and second millennium BC. Lastly, an attempt at the chronological and cultural division of the so-called Bronze Age in northern Portugal in the second and early first millennium BC is made.

Acknowledgement

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Prehispanic Agricultural Terrace Systems in the Chicha-Soras Valley, Peru

by HELEN C.M. KEELEY* and FRANK M. MEDDENS**

This paper sets out to review the prehistoric development of the terrace systems in a little-known part of the highlands of Peru.

LOCATION

The Chicha-Soras river valley is situated on the present border of the departments of Apurimac and Ayacucho about midway between the towns of Puquio and Andahuaylas in the Southern Highlands of Peru (Fig. 1). The Chicha-Soras river joins the Rio Pampas which in turn becomes a tributary of the Apurimac. The archaeological research on which this paper is based (Meddens, 1985, 1989, 1991) covered some 18 km of the length of the valley.

GEOLOGY AND GEOMORPHOLOGY

The region comprises a tableland intersected by steep-walled valley systems, of which the Chicha-Soras is one. The valley varies in altitude from 2,600 to 4,000 m from the base of the basin floor in the north to the tableland on either side. The rocks are of volcanic origin and deposits consist of layers of basalt and tuff. The lava deposits are thick and extensive, suggesting long development of multiple-fissure extrusions with occasional shorter periods of more centralised explosive activity (Eastman, 1980). The basalt is columnar, indicating very fluid flow and slow cooling. This particular volcanic activity dates back to between the Ordovician and the Devonian ages (*Mapa Geologico Del Peru*, 1975).

The valley was formed by the action of water, frost and slope failure; there is little evidence of erosion on the tableland. The soil cover over the basalt on the plateau is often thin but soils in the valley show stronger development, most particularly on layers of tuff. The valley sides are steep due to the rapid erosion of the tuff deposits. Large boulders, originating from rockfalls of the basalt, are important features of the basin. The presence of thermal springs near the village of Larcay indicates that the area remains part of an active geothermal centre (Eastman, 1980).

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121

CLIMATE

Rainfall measured at Pampachiri (3,364 m) between 1964 and 1976 ranged from 450.0 to 1,685.5 mm per annum, with a mean of 980.6 mm and a standard deviation of 474 (information from the Servicio Nacional de Meteorologia y Hidrologia) which indicates the potential variation in precipitation from year to year. There are pronounced dry and wet seasons, i.e. dry from April to November and wet from December to March. Temperature varies considerably between day and night, with frequent night frosts, particularly during the dry season.

VEGETATION AND AGRICULTURE

The area incorporates a number of different ecological zones. On the altiplano or

Fig. 1   Map of Peru with location of the Chica/Soras valley.

'puna' at 3,800 m and above (which conforms with 'el Paramo humedo subalpino' of Tosi, 1960) highland grasses predominate. The tableland is too high for cultivation and animal husbandry is the main activity there. Domesticated Andean camelids—llama and alpaca—are the principal grazing animals; occasionally sheep, goats, cattle and horses are herded.

Within the Chicha-Soras valley arable agriculture is important and the rearing of livestock takes place. On the upper slopes of the valley (3,400–3,800 m) grasses, cacti and low shrubs predominate, although some trees (especially the introduced eucalyptus) occur in more sheltered areas. This zone conforms to the 'pradera o bosque humedo montano' of Tosi (1960). Sheep, goats, cattle and horses are kept. Tubers, such as potatoes and ulluco, are the main crops. Some maize, quinoa and tarwi are grown in the more sheltered fields. Any surplus produced generally comprises sheep, cattle and potatoes, which are taken to town or city for sale (usually to Andahuaylas or Lima).

The lower slopes (2,600–3,400 m)—equivalent to Tosi's (1960) 'sabana o bosque seco montano bajo'—demonstrate more pronounced relief, with steep slopes where shrubs and
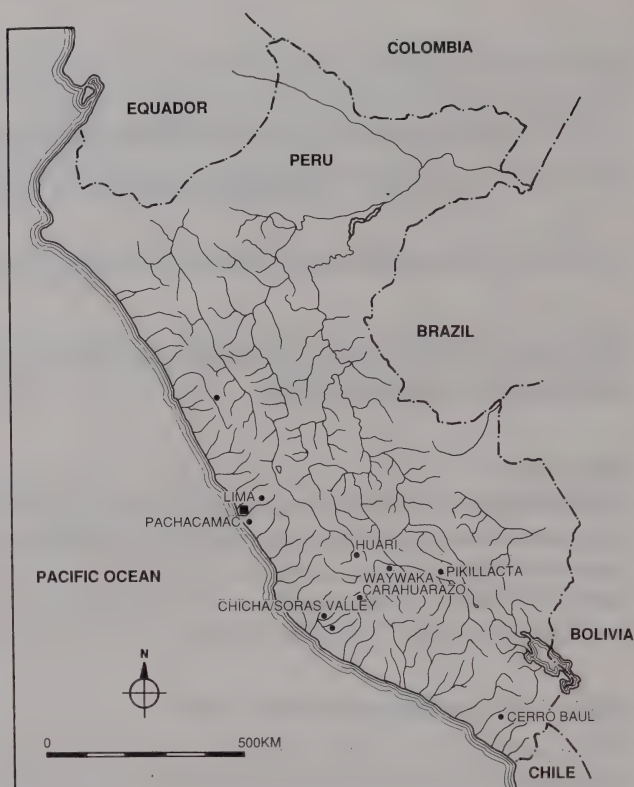
trees are more common. The common domestic animals are sheep, goats, cattle and horses; and the main maize crop is grown in this zone.

Agricultural work begins in March, at the end of the rainy season, when the initial ploughing is done. A second ploughing is carried out during September and October. Once the fields are prepared the planting is done; the lands are tended in December and January and crops are harvested in May and June. Fields are cultivated once every six years. The animals have their young during the wet season when food is plentiful. The fields are terraced and the cultivated ones are irrigated, mainly from natural water sources which principally consist of small tributary streams of the Chicha and Soras rivers (Meddens, 1985). Man-made irrigation canals are rare, a factor which somewhat limits the agricultural potential of the area at present.

Fig. 2   Map of the Chica/Soras valley with some sites of importance mentioned in the text.

The Minga form of labour organisation still functions in the villages, especially during the maize and potato planting and harvesting—labour duties are performed and food and drink are provided by those profiting from the work.

At present the principal maize crop for the area surveyed comes from the northern part of the valley. For instance, the main maize crop of the village of Pampachiri, located in the southern sector of the valley at 3,362 m above sea level, comes from the area below Huayana some 17 km to the north, ranging in altitude from 2,600 to 3,000 m above sea level. The people of Pampachiri have rights to lands at Huayana where they plant and harvest the crop
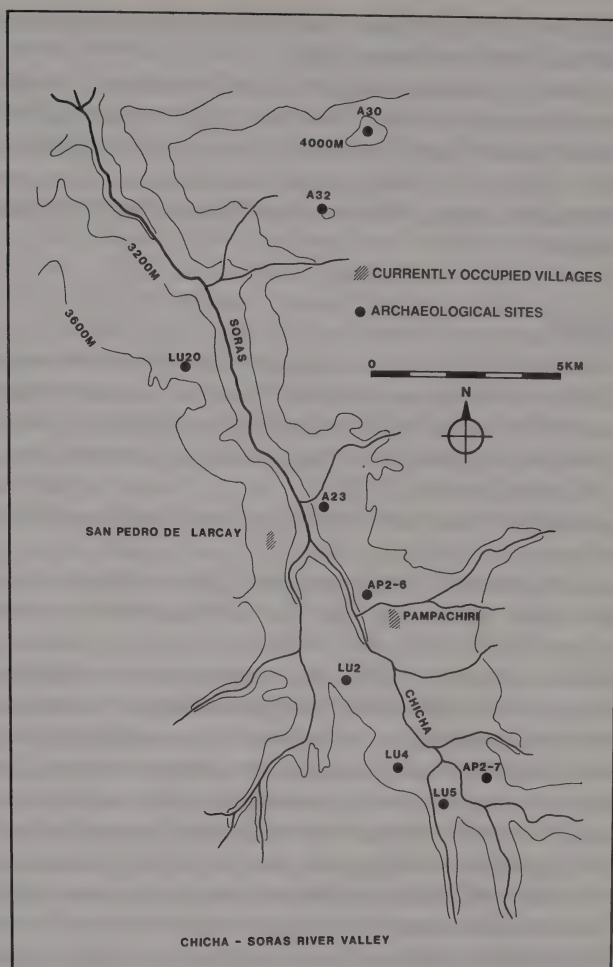
themselves. These aspects clearly indicate the persistence of a system of vertical control in the local economy (Murra, 1980).

Barter-style arrangements exist between the Pampachiri district and the Vilcabamba area in the department of Cuzco, from which coca in particular is obtained and to a lesser degree coffee, achiote, cane, cattle hides, oranges, avocados and chirimoyas (custard apples). These are exchanged for dried, smoked and salted meat, potatoes, wheat, wool, cheese, ropes, camelid fat, horses, cows and pigs (Flores, 1985). The antiquity of this exchange link is unclear.

SOILS

The soils of Peru have not been mapped in detail but national maps have been produced by the Faculty of Agronomy, Agrarian University of La Molina (Zavaleta, 1964) at a scale of 1:3,000,000 and by FAO/Unesco (1971) at a scale of 1:5,000,000. Generalised descriptions of broad soil associations have been given by Drosdoff et al. (1960), Wright and Bennema (1965) and Zavaleta (1974).

Drosdoff et al. described two soil associations most likely to relate to the Chicha-Soras valley:

(a) The Andean Valley Association (1,800–3,600 m)—the valley floors are generally 2 to 5 km wide with a narrow strip of alluvium between two strips of colluvial and alluvial terraces broken by the alluvial fans and terraces of tributary streams. The soils of the steep hillsides with slopes of 20 to 50 per cent or more are lithosolic. These soils have an extremely thin profile in which the soil is mixed with angular fragments of parent rock. The A horizon is weakly differentiated, stony and often low in organic matter (Wright and Bennema, 1965). In many places accelerated soil erosion is a serious problem. The soils of the terraces and upland where relief is moderate are usually deep, dark reddish-brown with medium to fine texture and little profile development; well drained except for local areas of hydromorphism. The soils are neutral or slightly acid when developed on volcanic parent material and are deficient in nitrogen and phosphorus and occasionally potassium. Organic matter is generally less than 2 per cent. These steepland soils are related to Brown Forest soils, Acid Brown Forest soils, Brown Mediterranean soils (non-calcic brown soils), Brunizems and Chestnut soils (Wright and Bennema, 1965).

(b) The Puna Association (3,600–4,100 m)—undulating to rolling grasslands above the Andean valleys. An important soil of the uplands is the 'Pardo Andino' (Andean Brown), an acid tussock grassland soil related to Acid Brown Forest soils but showing intergrades to subalpine 'Paramo' soils (Wright and Bennema, 1965). In areas of calcareous rocks, Rendzinas, Calcic Brown Forest soils and Grumosols occur, with related steepland soils. The soils of the Puna Association are generally high in organic matter, moderately to strongly acid and low in available nitrogen and phosphorus.

ARCHAEOLOGY

For the two earliest periods of human land-use in the area evidence for terracing is lacking.

A Preceramic (?–1800 BC) presence in the area was indicated at two sites. Strata containing lithics, separated from pottery-bearing deposits by sterile layers were present here. The first location was in a road cut at the site of Chiqna Jota where, 3.5 m down the profile, an obsidian working floor with very high densities of waste was situated. The second was observed in a looters' pit at the site of Imglesia Chayoc where, 1.2 to 1.5 m down the profile, basalt and andesite artefacts were present.

The earliest dated evidence for occupation in the Chicha-Soras valley dates to the Initial Period (1800–900 BC). Four sites were located with surface pottery in the Muyu Moqo C/D style (Grossman, 1972) (Fig. 2). Site A23 is the principal Initial Period site. Although terracing was present at the A23 site no direct link can be made between it and the surface scatter of pottery. At Andahuaylas, at a partially excavated Muyu Moqo C/D settlement site, no evidence for terracing was detected (Grossman, 1983).

The earliest terracing is associated with a site dating to Middle Horizon Epoch 1A (AD 550–700). This site, Yako (Lu2), is the only Middle Horizon 1 site found in the survey area (Meddens, 1985, 1989, 1991). For Middle Horizon Epoch 2 (AD 700–800) the number of sites increased from one to seven. For Middle Horizon 3–4 (AD 800–1000), 10 sites were discovered; for the first half of the Late Intermediate Period (AD 1000–1476)—Late Intermediate Period 1 (AD 1000–1200)—the number rises to 14. Even though terracing is notoriously difficult to date, it seems likely that the systems found around and associated with these sites formed an integral part of the increase in the number of sites and intensification of land-use taking place over time.

It is during Late Intermediate Period 1 that the northern sector of the valley (north of San Pedro de Larcay) first shows evidence of prehistoric occupation and land-use. This may be related to the fact that this part of the basin has a more pronounced topography. The valley slopes are steeper and less land is directly available for settlement and cultivation. The northern part of the valley would therefore have demanded a comparatively greater labour input than the southern sector to make much of it available for occupation and agriculture. Within the survey area large-scale terracing of the slopes was advantageous over much of the area north of San Pedro de Larcay to increase its productiveness. Approximately 26 per cent of the land located north of San Pedro de Larcay could be cultivated without terracing; the rest benefited from terracing or the terrain was unsuitable for agriculture. For the area south of San Pedro de Larcay 50 per cent was directly accessible for agricultural purposes.

During the second half of the Late Intermediate Period, Late Intermediate Period 2 (AD 1200–1476) the number of sites increases from 14 to 21. A change in settlement location occurs. Sites are now established on hill and mountain tops. Auquimarca and Puyka constitute examples of this type of site location. These hill and mountain tops would have been easily defendable from any attacker. In this respect it should be noted that a shift to hilltop settlements occurs throughout the Rio Pampas area around this time, a shift which may be related to increased warfare (Lumbreras, 1974). All these sites are extensively terraced in order to make the hill and mountain tops accessible for settlement.

Not only does the number of sites in the Chicha-Soras valley increase during Late Intermediate Period 2, but some sites are much larger than any of those encountered for the preceding

periods. The site of Auquimarca (A30), the largest in the region, has between 1000 and 1100 standing circular structures.

For the Late Horizon (AD 1476–1534) seven to eight sites were recorded. This apparent reduction in the number of sites from the preceding Late Intermediate Period 2 may be explained in various different ways. The sites included in the Late Horizon sample are only those which had associated polygonal ashlar masonry and/or Inca-style pottery. All sites of this period contained in their assemblages Soras- and Arqalla-style pottery (Lumbreras, 1974; Meddens, 1985, 1992), which characterised the preceding Late Intermediate Period 2. Some of the sites dated to Late Intermediate Period 2 may therefore have had a continued Late Horizon occupation, but were not included in the Late Horizon sample as they lacked both polygonal ashlar masonry and Inca-style pottery. A differential distribution of these features during the Late Horizon might result if they were limited to specific sections of the population. Evidence for this type of distribution pattern has been noted before in work carried out in the area around Huanuco Pampa (Morris and Thompson, 1985). There is one indication, in the case of the Chicha-Soras valley, that classical Inca-style pottery had a wider distribution in a low status, suggesting dating to the Late Horizon at the site of Chiqna Jota where a number of Inca-style pieces imported from the Cuzco area were present (Meddens, 1992). However, the reduction in the number of sites could also reflect a real reduction in population, as various chroniclers indicate that the Inca conquest of the Soras area was a violent one (Cieza de Leon, 1971; Guaman Poma, 1980).

The terracing found in the valley is clearly not exclusively dedicated to agriculture. Terraces constructed to allow the building of occupation sites, defensive terraces, terraces associated with roads, as well as ones combining a number of roles, can be found.

It should be noted that throughout the prehistoric period the use of valley irrigation canals is rare. They are first associated with sites or sectors of sites dating to Late Intermediate Period 2, never number more than five, and none is longer than 300 m.

AGRICULTURAL TERRACES IN THE CHICA-SORAS VALLEY

Terracing is one of many alterations to the environment by human societies. This type of field system represents a large labour investment and is indicative of complex hierarchical social organisations. Terraces are places where man-made soils are built, retained and used, and these accumulations of anthropic soils often have long histories of use. In the Chicha-Soras valley all the land that could be terraced has been, although not all at the same time. Terracing in the New World has been reviewed elsewhere (Keeley, 1984, 1985, 1988) and has been discussed in detail by Donkin (1979), although he makes no specific reference to the Chicha-Soras area.

A number of sections were dug through terraces associated with two sites in the Chicha-Soras valley—at Yako (Lu2) and Chiqna Jota (Lu5). Selected profiles are described below.

Yako is the only Middle Horizon 1 site known in the valley. It is located on the west bank on a large natural terrace and occupies about 0.65 hectares. The site is extensively terraced. One of the terrace walls is bonded with a wall of one of the structures which indicates its

contemporaneity with this terrace and the terracing system. There are no irrigation canals and the nearest source of water is 45 m below the site (Meddens, 1985, 1991). The terrace system was well constructed, being built on a gradual incline (about 10 degrees at the top to 30–40 degrees on the steep sides, where narrow terraces were used), following the contours and tending to face east. Rainfall and/or run-off from the altiplano appears to have been the main source of water, and the natural slope of the terrain would have caused the water to gradually filter through the terraces.

Soil Studies at Yako

Three pits were dug to sample two of the terraces. The top terrace, adjacent to buildings at the top of the slope, was about 10 m wide and 25 m long, and the terrace immediately below was about 15 m wide and 20 m long. The retaining walls consisted of roughly dressed fieldstone, the top retaining wall being 0.5 m wide and 1 m high. The lower terrace had a front wall about 2 m high but the north section of this wall was much lower, at about 0.5 m, and seems to have risen partly above the surface of the terrace. The terraces faced east and were flat. The section through the front wall of the upper terrace revealed an unusually shallow profile underlain by weathered volcanic tuff. The mid-terrace profile was deeper and contained fragments of burnt bone and charcoal to a depth of 1 m. The section through the wall of the terrace below (Fig. 3)[1] was much deeper.
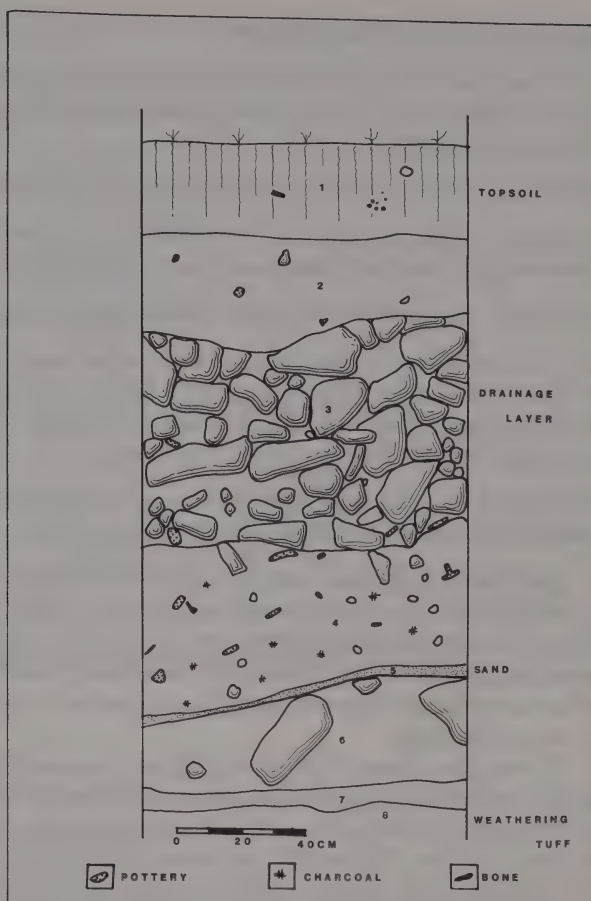

Fig. 3   Terrace section at Yako, pit 3, east profile.

[1]With some of the illustrated profiles the number of described layers does not coincide with the number of illustrated layers (for example Fig. 3). This is because some of the layers did not stretch across the whole of the excavated test pit to become visible in the profiles.

The section was described as follows:

0–30 cm   Dark brown (10YR3/3) friable coarse loamy sand with weak medium sub-angular blocky structure; many roots, coarse to fine fibrous; stones 10 per cent, gravel to medium-sized (mainly gravel).

30–50 cm   Similar to above but more gravelly (stones 20 per cent) and roots common.

50–60 cm   In places only, above the rubble. Similar to above but fewer stones (10 per cent); roots emerge at this level, many were coarse to fine fibrous.

60–120 cm   Loose unconsolidated rubble consisting of large boulders and smaller stones.

120–155 cm   Friable coarse sandy loam, similar to a dark layer noted in the mid-terrace profile; black and very dark brown (10YR2/2) mixed with some more yellowish-brown material (20 per cent) and pieces of weathered rock, pot, bone and charcoal; weak medium sub-angular blocky structure; stones 10 per cent.

155–160 cm   Yellowish brown coarse loamy sand mixed with darker material from above; friable with weak medium angular blocky structure; stones 20 per cent, mainly gravel.

Apart from the uppermost profile, which may have been unusual because of its proximity to the buildings, these soils showed considerable evidence of anthropogenic modification. The mid-terrace soil contained bone and charcoal to a depth of 1 m, probably incorporated during cultivation, and the lower profile was clearly almost entirely man-made, presumably to increase soil depth in order to provide a flat surface for cultivation and to facilitate drainage to the terraces below.

The analytical results (Figs. 4 to 6, HK1 to 3 sequentially) showed an increase in pH with depth in all profiles, suggesting that the upper layers had become base-poor through leaching and/or erosion. The topsoil in each profile contained more sand in relation to silt and clay than the lower horizons. This could be the result of erosion of fine material and of clay movement (lessivage) down the profile, as suggested by the noticeable increase in clay with depth.

The careful construction of the Yako terraces and the engineering and labour input needed in the building of this system indicate a degree of planning and control characteristic of societies with a complex hierarchical structure. The terrace system on this site was linked into others further up and down the valley suggesting possible contemporaneity of these systems. Together they cover an area of approximately 20 hectares (Meddens, 1991).

Even with a six or seven year crop rotation, this would result in yearly cultivation of approximately 3 hectares, clearly more than would be needed to support the occupants of the three-0p buildings making up this site (Fig. 7). That there were no more than these three buildings was confirmed by the analysis of variations in the density of cultural surface material associated with the site (Meddens, 1985). The amount of land under cultivation was more than could have been maintained and cultivated by the inhabitants of the place. This suggests that cultivators were brought in seasonally and that any surplus produced would have served a population outside the immediate valley. These could have been pastoralists from the surrounding altiplano or administrators resident in administrative centres located in
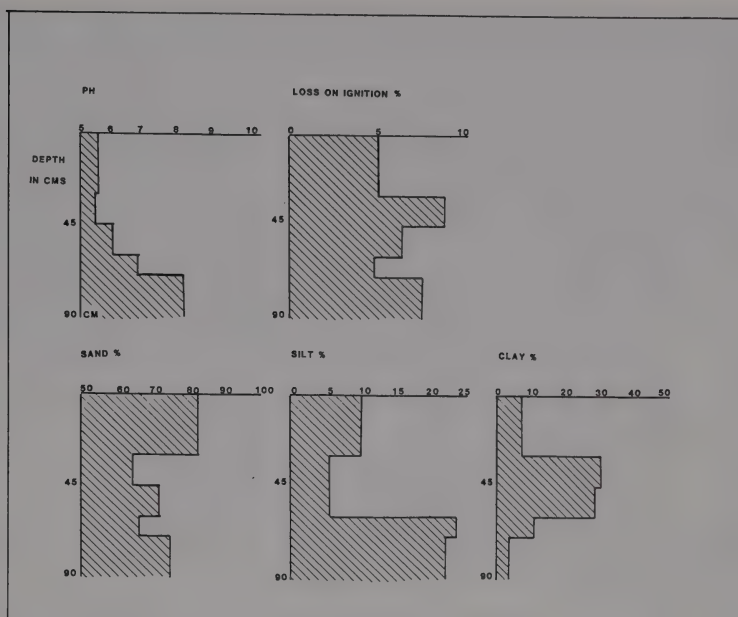
Fig. 4   Organic component, loss on ignition data from upper terrace section, Yako (HK1).
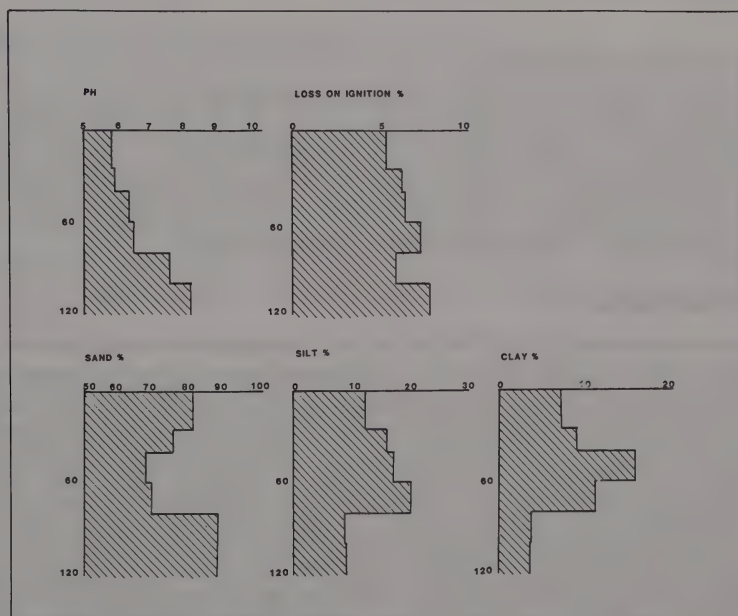

Fig. 5   Organic component, loss on ignition data from mid-terrace section, Yako (HK2).
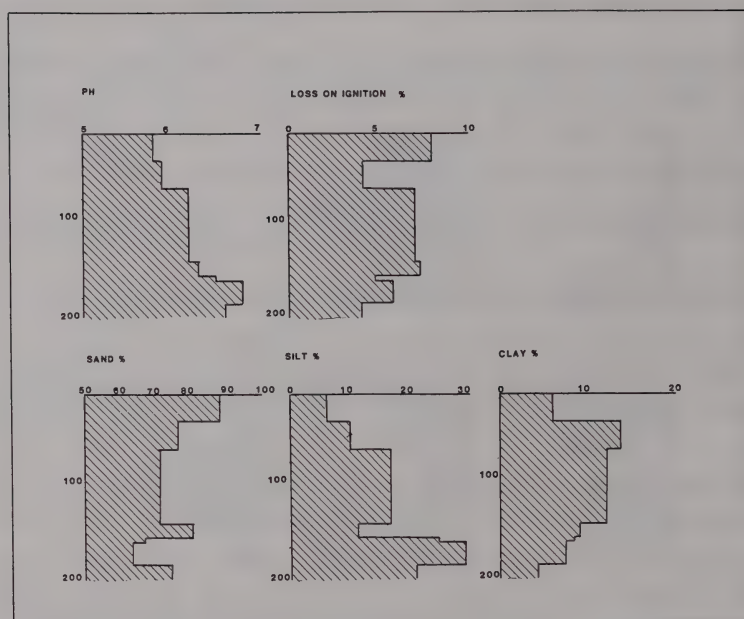
Fig. 6   Organic component, loss on ignition data from lower terrace section, Yako (HK3).

neighbouring valleys, such as Jincamocco in the Carahuarazo basin (Schreiber, 1978). Alternatively, the cultivated area may have supported a population resident in the valley outside the sampled area (18 km of the length of the valley). Or it may possibly have sustained populations settled even further afield. One more possibility is that Yako and its associated agricultural system was part of a vertical economy, with seasonal occupation by pastoralists who grew their crops there but only maintained a small, semi-permanent or permanent population in the valley outside the planting or harvesting seasons.

Soil Studies at Chiqna Jota

Chiqna Jota was established in the Middle Horizon 2 period and occupied up to the end of the Late Horizon (Meddens, 1992). The site lies near the confluence of the Chicha and Pachachaca rivers. It includes a hill on the north-west margin and is extensively terraced—some of this terracing dating back to Middle Horizon 2. A gallery-type structure dated to Middle Horizon 2 used a terrace wall as its own back wall. Corbels to support the roof, keyed into both the terrace wall and the facing wall of the structure, were clearly built as part of the original construction, rather than constituting a later alteration. The last time that parts of the site are known to have been cultivated was about 1972 (Meddens, 1985). A large natural depression in the eastern sector had been extensively modelled and terraced, with a large plaza at the lowest point. On the eastern side a steep drop of 80 m to the Chicha river bed constituted the

Fig. 7   Portion of wall of the principal building at Chiqna Jota.

eastern limit of the site. The open side of the plaza and the adjoining terraces face the morning sun. The terraces below Chiqna Jota are difficult to date securely but are thought to be Late Intermediate Period 1 (AD 1000–1200) at the earliest, based on pottery excavated there. The terracing at this site fulfilled a variety of roles, both residential and agricultural.

During a brief visit to the area in 1980, a section was cut through a terrace below Chiqna Jota and the results reported (Fig. 7) (Keeley, 1981). Subsequently, in 1982 a number of pits were dug in the plaza area and the sequence begun in 1980 below the site was extended.

The Plaza

A section was cut through a terrace on the east side of the flat and west-facing plaza. In general the terrace walls were very eroded. The upper terrace was about 30 m wide, 25 m long at the back and 32 m at the front. The back wall was 3 m high and the front wall 2 m, the natural slope was about 5 degrees. A disarticulated human skeleton was found in and under the rubble at the front of the wall; much of it was missing although fragments of all major body parts were present. The skull was found in a small, round hole underlying the rest of the bones. The remains appear to have been interred as indicated by the cut in which the skull had been placed. The small size of the fragments and the incompleteness of the skeleton suggest that

it was disarticulated prior to burial. Its nearness to a possible *Usnu* (ceremonial platform) in the plaza (Meddens, 1985, 1991) proffers the possibility that the individual represented an offering or sacrificial victim. Unfortunately the bone was not in sufficiently good condition to permit the identification of butchery marks. Similarly incomplete and damaged human skeletal remains were found in, amongst and under rubble of wall collapse excavated at Yako (Meddens, 1985).

The soil profile (Fig. 8) was very deep, reaching below 210 cm, and had a slightly compacted layer at about 1 m containing fragments of bone and pot, below which mottling increased possibly as a result of water percolation.

In the mid-upper terrace pit a whole pot was found underlying a very large boulder. It had been placed in a pit which had been capped by the rock. This vessel was in the Soras style, a variant of the Chanka style (Lumbreras 1974; Meddens 1985, 1992) dating to the second half of the Late Intermediate Period. It probably constituted an offering as it appears to have been buried intentionally. Pottery fragments were found down to the base at 1 m in this profile but only to 32 cm in the section through the back of the lower terrace (Fig. 9). Particle size distribution, organic matter content (loss on ignition) and pH are shown in Table 1.

pH showed little variation with depth but the distribution of sand, silt clay and organic matter fluctuated quite considerably throughout this profile, confirming the man-made nature of these terrace soils and suggesting that material from a number of different sources had been used to fill the terraces. It is possible that in some cases the material was chosen deliberately for particular purposes, e.g. in PI sample XIII had an unusually high clay content and may have been selected to slow down water loss from the base of the profile; this must, however, remain a matter for speculation. What is clear is that a complex technology was used in
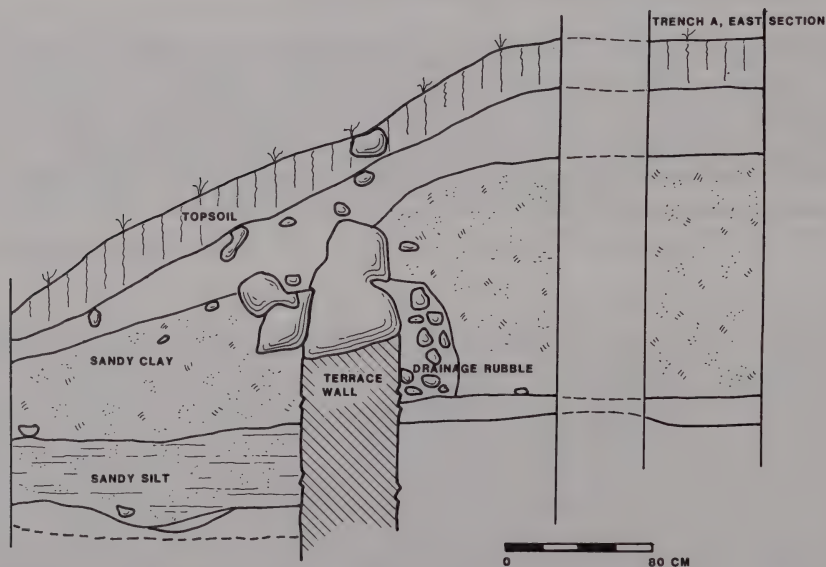

Fig. 8   Chiqna Jota terrace section, trench A, North profile.

constructing these terrace soils, in which it seems likely that they were prepared and enhanced for a variety of reasons, not all of which were necessarily related to crop requirements. It should be noted, moreover, that in an area which today, at least, suffers from variable rainfall it is likely that a considerable effort will have gone into water retention qualities and drainage demands of particular terraces and terrace systems.

The limited variation of pH in the plaza profiles and adjoining terrace may indeed reflect a non-agricultural use of this area. A lack of cultivation would have produced this kind of pH distribution, although other explanations are also possible.

Terraces below Chiqna Jota

In 1980 a section was cut through a terrace about 6 levels down from the apparent limit of the principal habita-

Fig. 9   Chiqna Jota terrace section, trench 1, B–2, South profile, front of plaza terrace.

tion component of the site (Fig. 7). The upper terrace sampled here was 12 m wide and the lower 15 m; the walls were approximately 1 m high. The fields faced west-north-west and the natural slope was about 15 degrees. In this profile there was a marked increase in clay content between 25 and 65 cm. The remainder of the profile was predominantly sandy in texture and low in organic matter, suggesting deliberate manipulation of the soil drainage regime.

During the 1982 season a large terrace five levels further down was excavated. It faced north-west and was situated west of a knoll associated with occupation remains north of the main site of Chiqna Jota. The terrace walls were 1 m high but very eroded. The terraces were level and situated on a natural slope of 3 to 4 degrees increasing to about 30 degrees towards the main sector of the site and 45 degrees to the west.

The analytical results for the section at the front of the terrace through the wall show that
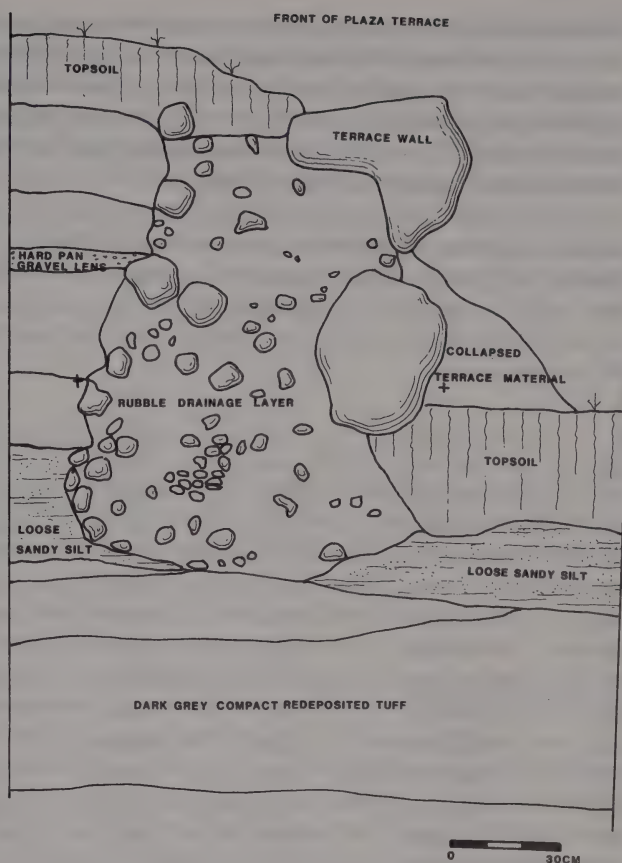
the sand, silt and clay contents varied considerably, confirming the man-made nature of the soil, and pH and available phosphorus content increased slightly with depth, suggesting that the upper part of the profile has been leached. Levels of nutrient elements were generally low, e.g. total nitrogen ranged from 1051.66 to 281.93 ppm (mean 496.19 ppm), total phosphorus ranged from 880.72 to 480.03 ppm (mean 646.51 ppm) and available potassium ranged from 644.73 to 185.52 ppm (mean 407.46 ppm).

Table 1. Analytical results for soil samples from the plaza.

Sample No.	Depth cm	loss on ignition %	Sand %	Silt %	Clay %	pH
PI: Front of terrace:						
I	0–30	4.2	79.9	7.5	12.6	5.7
II	30–45	4.4	83.3	8.7	8.7	6.0
III	45–47	4.6	73.9	13.1	13.1	6.0
IV	47–60	6.6	80.1	14.5	5.4	6.0
V	60–62	5.1	66.3	22.9	10.8	5.9
VI	62–73	3.1	72.8	20.8	6.4	6.0
VII	73–75	6.0	53.8	34.7	11.6	5.9
VIII	75–92	5.8	72.8	15.9	11.4	6.4
IX	92–102	4.5	64.3	17.9	17.9	5.6
X	102–125	4.1	74.3	12.9	12.9	6.4
XI	125–130	4.5	63.0	15.9	21.1	5.9
XII	130–160	3.6	65.4	27.7	6.9	6.4
XIII	160–175	6.3	59.3	7.4	33.3	6.5
XIV	175–210	5.2	56.2	35.6	8.2	6.3
XV	>210	5.5	49.5	34.5	22.0	6.0
PII: Mid-terrace						
I	0–35	4.2	75.8	12.1	12.1	5.0
II	35–48	3.0	85.3	4.2	10.5	5.8
III	48–60	8.5	73.0	11.2	15.8	5.4
IV	60–70	4.4	91.2	0.0	8.8	5.7
V	70–90	6.8	74.2	0.0	25.8	5.9
VI	90–100	7.6	38.0	40.3	21.7	6.3
PIII: Back of terrace						
I	0–23	23.4	78.0	8.0	14.0	4.9
II	23–32	4.7	74.8	18.5	6.9	5.2
III	32–45	4.3	32.8	21.4	45.8	5.6
IV	45–64	8.8	17.1	75.1	7.8	6.2
V	>64	7.0	40.5	38.7	20.8	6.1

In the mid-terrace profile there was less evidence of leaching. Sand, silt and clay contents indicated some discontinuity within the profile, with perhaps the first metre of the profile having been added to, or modified, during construction of the terrace. Levels of nutrient elements were again low (e.g. mean total N 453.34 ppm, mean total P 556.47 ppm and mean available K 305.85 ppm). Apart from fluctuations in available phosphorus content, the profile at the back of the terrace showed particle size distribution, pH and organic matter contents similar to the mid-terrace profile, again suggesting modification of the upper metre or so during terrace construction. Nutrient levels were similar to those in other profiles (e.g. mean total N 493.32 ppm, mean total P 713.25 ppm and mean available K 421.95 ppm). These terraces thus clearly showed not only the engineering effort in shaping the terrain and building the retaining walls but also the labour invested in importing earth and altering the nature of the soil.

DISCUSSION AND CONCLUSIONS

The agricultural terraces at Yako and Chiqna Jota were clearly very carefully constructed and in many cases, particularly at the front of the terraces, the soils were almost entirely man-made. The analytical results demonstrate the variability of the deposits filling the terraces and there is some evidence, as in the plaza terraces, to suggest deliberate use of certain types of soil material in order to influence water drainage. Levels of nutrient elements in the soils were generally quite low and some profiles showed evidence of leaching. Organic matter contents were adequate in many cases and it would probably be fairly easy to grow crops on these terraces, most of which are now abandoned, given suitable fertilizer inputs. Eroded terrace walls would need to be rebuilt but the limiting factor would certainly be the availability of water.

The lack of an integrated system of irrigation canals here is interesting and unusual in an area which has been so thoroughly terraced, suggesting that there was no pressure on the ancient cultivators to create such a system in order to increase their output. This may have been due to a wetter climate which allowed a longer period for wet season agriculture, but this remains to be substantiated. In this respect it should be noted that the limited irrigation systems increase in frequency and complexity through time up to the Late Horizon. Natural water sources from the altiplano catchment may have been used to augment rainfall. This seems to be implied by the carefully constructed drainage layer in the Yako terraces. Unfortunately little archaeobotanical information is available to provide evidence of crops grown in the Chicha-Soras valley during the Middle Horizon and Late Intermediate periods. Small amounts of carbonised maize kernels and cobs were recovered in excavation from Middle Horizon votive deposits. Maize is at present being cultivated with varying degrees of success, depending on altitude zone and microclimate, throughout the sampled area. It is to be expected that a proportion of the terraced fields would lie fallow each year and that crop growth was subject to the limitations imposed by the use of rainfall agriculture.

The valley north of the modern village of Larcay, in the study area, showed no evidence for agricultural terracing until the Late Intermediate Period. It then became an integral part of the agricultural system functioning within the basin and the valley slopes there were densely terraced. This part of the valley has a more pronounced relief. Terracing here would have both

conserved the soil cover on the slopes and opened up the possibility of more extensive maize cultivation.

Terracing in the Chicha-Soras valley began in Middle Horizon 1 as shown by the site of Yako. This would have produced an agricultural surplus used to support an extra-site population. Expansion occurred during Middle Horizon 2 and most terracing in the valley had been done by the end of this period (e.g. Chiqna Jota). This was then followed by gradual growth into the Late Intermediate, when more peripheral areas were terraced. Terraced agriculture probably did not support communities outside the local region after Middle Horizon 1. The few irrigation canals which have been found are associated with Inca or very late Late Intermediate occupation in the valley.

Abstract

This paper reviews the structure of terracing in the Chicha-Soras valley in southern Peru. It presents the changing land-use in a diachronic overview within the context of our current knowledge of Peruvian culture history. In this context the existence of state control is recognised within the pattern of terracing observed. The absence of large-scale irrigation systems within the complex terraced infrastructure is reviewed as is the gradual increase in use of terraced land from Middle Horizon Epoch 1 through the Late Horizon.

Acknowledgements

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THE S.C. FACTOR ON THE
ROMAN NORTHERN FRONTIER

by ANNE S. ROBERTSON*

Professor Anne S. Robertson has devoted many years of study to, among other things, Roman Archaeology in Scotland, the publication of the Roman coins in the Hunterian Collection, and to the definitive publication of Roman coin hoards in Roman Britain. This last work was started as an M.A. at the newly emerging Institute of Archaeology in 1937 and the final text is now being prepared by the Royal Numismatic Society as a Special Publication.

She wrote the following article some years ago as a contribution to a seminar on the northern frontiers of Britain but had doubts about putting it into print. I am very pleased that she has now agreed to let it go forward, for two main reasons. It says something important about the interpretation of coin finds—we must allow for human quirks in the archaeological record. And it allows out a glinting sense of humour, almost mischief, which she has restricted in her many 'scholarly' works, but let out in gentle doses in her dealings with her colleagues.

Richard Reece
Institute of Archaeology

There will be some old-timers here, of whom I count myself one, who will remember that a few years ago a colleague of mine persuaded me to present a paper of his, giving evidence which convinced him that the latest occupation of the Antonine Wall took place in the Byzantine period. This colleague has turned up again. Just rushed in one day, sat down uninvited, and said: 'I want you to present to the Northern Frontier Seminar what I believe to be the S.C. factor on the Roman Northern Frontier in Britain.'

A.S.R: But I've already told Bill Hanson that I'm hoping to talk about what I consider to be the S.C. factor on the Roman Northern Frontier.

Colleague: Yes, but you've told him that you only wanted five minutes, and I know what you mean by the S.C. factor. You mean the problem posed by Roman bronze coins with S.C. on them. I know, too, that you only want to talk for a very short time, because although you've seen all the available coins from Scotland, you still have to examine and re-examine the coins from Hadrian's Wall in the Museum of Antiquities, Newcastle. You only want five minutes to put a few questions, and you've been given fifteen minutes on the programme. That leaves plenty of time for me to talk about what I consider to be the S.C. factor. I tell you

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what we'll do. I'll talk about my S.C. factor for a little while, then I'll let you talk about yours, and then I'll finish off with mine. Don't trouble to thank me. I don't mind doing it.

A.S.R: What do you consider to be the S.C. factor?

Colleague: Remember when we were talking about Byzantine occupation, we mentioned 'what-elses': What else could these be but barracks? What else can this be but Flavian? What else could this be but a tower? Recently I've been considering categorics and paregorics— 'the Romans must have done this'; 'the Romans would never have done this'; 'the Romans always did this'; 'the Romans never did that'. As a result, I've come to the conclusion that a factor that must always be taken into account is the S.C. factor—the sheer cussedness factor. Now, nobody has found more sheer cussedness than you have. Just look at Cardean. Anybody can tell that Cardean was laid out with a left-handed gun.

A.S.R: You mean a left-handed *groma*.

Colleague: There you are. You admit it yourself. (Slick as a TV interviewer, putting words into his interviewee's mouth.) The fort was laid out with a left-handed *groma*, not as a rectangle but as a parallelogram, and even the internal buildings were laid out with a left-handed *groma*. How do you think the soldiers felt, when they went into a barrack block and found the wall running away from them, off to the left? And look at that granary of yours, in the central block, just inside the south gate. At first you discovered thirteen parallel trenches, and between three pairs of trenches you found six large postholes. Later, you found eight more parallel trenches, and another row of postholes, covered by a seventeenth or eighteenth century road. And what about that building you found next to the granary, with its eight little rooms?

And what about overall cussedness at Cardean? With all these wooden buildings in the barracks area and in the central block you didn't find a single scrap of carbonised or charred wood, in spite of having a palaeobotanist temporarily resident on your excavating team. Don't you think that's as cussed as they come, when the Romans are supposed to have left all their timbers in place?

And Cardean is not the only site where you have found cussedness. It seems to follow you around. Think of the Roman road, with its signal towers, on the Gask Ridge. It doesn't even know which way to look. Some of the signal towers face to the north, and some to the south. Fifteen miles or so of Roman road in mid-air. Do you call that a frontier? It leaves me speechless.

A.S.R: Good! I can talk about coins for a little while. There's something very strange about Roman bronze coins found in Scotland, so very strange that I shall have to check the strangeness with other Roman site finds. The bronze coins which Agricola's army brought into Scotland were minted in the reign of Vespasian, either in Vespasian's name or in the names of his sons, Titus and Domitian. All were minted in Gaul.

Later, there came bronze *Asses* of Domitian, issued in AD 86, all unworn, of which the number is still growing: seven from Inchtuthil, one from Dalginross, one from Stracathro, two from Dr Maxfield's recent excavations at Camelon (which she has kindly allowed me to include), another possible from Camelon, two from Crawford, at least three, possibly six, from Newstead, and possibles from Castledykes and Barochan. These are all newly minted.

Never yet has one been recorded from Scotland which is worn.

The familiar argument is that a consignment of newly minted *Asses* of AD 86 was sent in bulk as military pay direct to North Britain from the mint at Rome or from an intermediate repository. The northern forts established by Agricola therefore began to be evacuated at the very end of AD 86 or shortly afterwards. In an attempt to discover how long afterwards, I have been, and still I am, searching for later unworn coins of Domitian from elsewhere. Meanwhile, as examples, there are newly minted *Asses* of AD 87 from two Roman towns in Southern Britain, Silchester and Verulamium. This shows that *Asses* of Domitian, of a later date than AD 86, were coming into Britain, and ought to appear in the coin record from Scotland, if the Roman army was being regularly supplied with new coinage. And if it was being so supplied, the date of the evacuation of the northern forts cannot be much after AD 86.

In strange contrast to the bronze coins lost during the Agricolan, or Flavian period there are comparatively few bronze coins of the second century recorded from Scotland. It is often said, categorically, that bronze coins could be lost far more easily, and remain lost far more readily than silver coins, because anyone who lost a silver coin would look for it, while anyone who lost a bronze coin might not look for it so energetically. In fact, there is only a handful of bronze coins of the Antonine emperors from Scotland, while there are many more silver coins. It may be that, for the end of the Antonine occupation in Scotland, we'll have to look more at the silver coins than at the few bronze coins. Once again, I am looking elsewhere, especially at sites on Hadrian's Wall, to see if the comparative scarcity of Antonine bronze coins can be paralleled.

The explanation may in fact be found in the alloy from which *sestertii* and *dupondii* were made. They were made from copper and zinc, whereas *Asses* were made from copper. Zinc was a very expensive metal to obtain, and although in the first century AD the proportion of zinc to copper in *sestertii* and *dupondii* was 20 per cent to 25 per cent (true Roman brass or *orichalchum*), by the time of Antoninus Pius the proportion had shrunk to about 3 per cent. Chemical analysis has shown that copper *Asses*, and zinc-alloy *sestertii* and *dupondii* were all being melted down together to get metal for new *sestertii* and *dupondii*. This could only be done if there was some machinery for gathering in bronze coins, and if so, such machinery would affect the proportion of second-century bronze coins in our numismatic record. It would appear to be very low.

Colleague: Very interesting, but let's look again at what is actually on the *Britannia Asses* of Antoninus Pius. There's something cussed here. The standard that Britannia is holding is not like the standard on the reverses of any other coins with Britannia seated left. I am in agreement with Lino Rossi, who has suggested that the standard on the *Britannia Asses* of Antoninus Pius is the standard of a *numerus Brittonum*.

There's something else I want to show you. (He started pulling things out of his coat pockets. Then he put his coat down on top of his briefcase, so hiding his briefcase.) There was a centurion early in the reign of Hadrian, who put down his bronze purse, with his cloak on top, as the fort rampart at Birdoswald was being built, and then he pulled his cloak away and left his purse under the rampart.

Now look at this picture. It shows the earliest (*not* recent) use of a mechanical excavator on

the Antonine Wall, 30 years ago at Polmont, where one group of archaeologists was looking for the stone base of the Antonine Wall, and was also looking for a fortlet, at the same time as another group was looking for a fortlet on Crow Hill between Balmuildy and Bearsden. You know, there is on the Antonine Wall, and on the Roman Northern Frontier generally, a sheer cussedness, which is not Roman, but modern. It takes the form of an intermittent selective dyslexia, which either cannot read or will not read certain things which it does not want to read.

(Meanwhile he was still pulling things out of his jacket pockets. Out came a dirty handkerchief with a little bunch of sixpences. I looked at it.)

Colleague: I'm saving these for my wife to put in the Xmas pudding. I used to save threepences, but they've gone out of use, and now I'm saving sixpences. There was a Roman soldier at Birrens who ought to have been paying his way with second-century *denarii*, but who actually had five base *denarii* of Mark Antony, saving them for what?

(He pulled something else out of his pocket, and out fell a little group of Roman playing men and melon-shaped beads. We looked at one another.)

A.S.R: Where did you get these?
Colleague: Where? I won them in a poker game; I can't remember where. I travel around so much, I only know I won them in a poker game. I don't suppose the man I won them from even remembers where he got them.
A.S.R: But do you realise that these are antiquities. You brought them back from somewhere, you don't know where, and you've dropped them here.
Colleague: And the Roman soldier said to his mate, 'Where did you get these?' His mate said, 'I've been on a purchasing tour with the quartermaster. I got into a game somewhere, and I won these and I brought them back.'
However, this is what I really wanted to show you. I've put all the S.C. factors I've found so far into a histogram. After all, every home should have a histogram. Give a histogram for Christmas to the man who has everything. The funny thing is, my histogram turned into a pictogram.

(He handed me a copy of the DIO MAPONI inscription from Birrens, with a drawing of a ?little Celtic dog below.)

A.S.R: That's not a pictogram. That's a celtogram.
Colleague: That reminds me (sliding away at once from the least hint of one-upmanship). I'll have to be on my way. I'm doing some research now on the cussedness factor in prehistoric times, the B.C. factor; and then I'm going on to the cussedness factor in medieval times, the A.C. factor. After that, I'm going on to the M.G.M. factor in Roman Northern Frontier studies. I'll want you to tell the seminar about that next.
A.S.R: Why don't you come and tell the seminar yourself?
Colleague: I don't like… (His voice sank to a whisper.) But what are you looking so shocked for?

A.S.R: That wasn't very kind, and it wasn't very polite.

Colleague: Kind (he laughed)! Polite (he laughed)! What's a quaint little old-fashioned lady like you doing in a place like this?

And then he bounced out and slammed the door. And then he opened it again very quietly. 'I've left my briefcase behind', he said.

CMS

Fig. 1   The DIO MAPONI inscription from Birrens, showing a ?dog.

DISCUSSION

WSH: I'd like to know what Professor Robertson's opinion is of the *groma* used for the construction of the forts at Newstead, in its first phase, and similarly Milton.

ASR: You're not really going to take this seriously?

WSH: The layout of the fort at Newstead is quite a well thought-out experiment. I find it difficult to draw it from memory and I get the feeling that at Milton someone was trying to copy the Newstead lay-out in just that way. On a slightly serious note, on your left-handed *groma*, if one examines modern or not-too-modern vernacular architecture, rarely do the buildings fit into neat categories of square or rectangular design.

ASR: You really do take this seriously. The same funny thing happened the last time— several of you started arguing about the impossibility of there being a Byzantine period on the Antonine Wall.

CMD: The greatest tragedy about last time was that it was never preserved on tape.

NMH: Can I ask a question about the more serious part of your talk? The question of the lack of bronze coins in the Antonine period. Could this not be contributed to by two factors—first of all, the fact that the silver *denarius* with inflation, was becoming a more popular coin by then than it was in the first century, and secondly, the effect of soil conditions on bronze coins. My experience of Scottish sites suggests that it can be very unkind to bronze coins and a lot of them may well have disappeared altogether.

ASR: You see, we're between two periods here.

1) The first century when as I have, I think, shown the pay appeared to be calculated and handed over largely in bronze.

2) The Severan period when pay was being handed over in silver—so we're probably at the period when it was partly in silver and partly in bronze.

But, my chief warning here is, it does not quite do to make a list of statistics or even to make a histogram because you have to look at what is behind the data. As one numismatist said, the evaluation of what is behind the statistics, not mere numerology, is what is required. And here I think we don't understand fully what is behind our own lists of coins. They are precious evidence. It takes time to read every single one of them. One gets tired of histograms and statistics—anyway, in military areas, the delivery of coins must have greatly affected the lists that we now have.

NMH: Would the smaller quantity of zinc in the material of which the coins are being made affect the rate at which they deteriorate in the soil?

ASR: I don't know if anyone has done this. It would be well worth trying. Of all the provincial collections I have seen, the collection from Hadrian's Wall is one of the richest in information, and one still to be fully exploited.

St Bride's and the City Churches Building Record

by GUSTAV MILNE and ANDREW REYNOLDS*

THE 1952–4 EXCAVATIONS OF ST BRIDE'S CHURCH (RMLEC)

The excavation of St Bride's church in 1952 (Fig. 1) was a landmark in the development of medieval archaeology, for this was the first parish church in England to be excavated in plan using modern archaeological techniques. The pioneering archaeologist responsible for this development was Professor W.F. Grimes, the director of the Roman and Medieval London Excavation Council (RMLEC) from 1946 to 1962, who subsequently went on to become the Director of the Institute of Archaeology (1956–73). He was able to show that a series of smaller churches had preceded the construction of the surviving building, by revealing the foundations of the older structures when he excavated beneath the nave. The occasion which precipitated this research opportunity was the devastation caused by the infamous fire-storm on 29 December 1940, a concerted attack by the German Air Force on the City of London, which resulted in the destruction of many buildings including Guildhall and nine churches. St Bride's was one of those unfortunate churches which was gutted during that raid. In 1952 the rector, the Reverend Cyril Moxon Armitage, appreciating the value of the opportunity presented by the unwelcome attentions of the Luftwaffe, invited the RMLEC to undertake investigations in the shell of the church before rebuilding began. Initially the aim was to locate a medieval crypt thought to lie beneath the north-east corner of the building, but Professor Grimes presented a persuasive case for the excavation of the whole internal area. The rector not only agreed to this plan, but offered to pay 1,000 guineas (£1,100) for the project. That generosity at a time of great austerity when funds were needed to rebuild the ruins speaks highly of his vision.

One of the results richly justified his action. For the first time the structural history of a parish church was revealed by archaeological investigation, from its modest pre-Conquest origins to its fully-developed fifteenth century plan with additional aisles and chapels. It was the latter building which was burnt out in the Great Fire of 1666, to be replaced by one designed by Sir Christopher Wren, built between 1671 and 1675 principally under the direction of his stone mason, Joshua Marshall. The famous steeple added in 1701–3 was itself rebuilt after being struck by lightening in 1764 (Godfrey, 1944).

One of the results of Professor Grimes' work was to show how closely the plan of the 'new' late seventeenth century church followed that of the fifteenth century building, for the Wren

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145

Fig. 1   Plan showing location of St Bride's church in relation to line of medieval features such as town wall, St Paul's and the Tower. Sites of other churches excavated by RMLEC marked: 1) St Albans Wood Street; 2) St Mary Aldermanbury; 3) St Swithun. (Chrissie Milne: London Archaeological Research Facility)

design re-used the base of much of the medieval walling as the foundation for the external walls. In addition, dismantled stonework from the old building was extensively used in the core and footings of the new work. As a consequence of that cost-effective seventeenth century approach to restoration, the foundations and lower courses of the superstructure of the medieval church survived, and are still visible in the crypts and vaults beneath the present-day church. It is these substantial remains which formed the focus of the 1992–3 recording project, which is described below.

THE 1992–3 RECORDING PROGRAMME AT ST BRIDE'S CHURCH (U.C.L.)

Although a substantial interim report on the 1952 excavations was published (Grimes, 1968: 182–97), the more detailed final report had not appeared before Professor Grimes died in December 1988. The Museum of London then acquired the field records associated with the 1946–62 excavations conducted by the Roman and Medieval London Excavation Council. That material, the Grimes London Archive, has now been sorted and catalogued by John

Pl. 1   St Bride's church, Fleet Street, EC4: Sarah Semple (U.C.L.) cleaning the walls of the late thirteenth to early fourteenth century crypt prior to recording the elevations. (R. Bartkowiak: London Archaeological Research Facility)

Pl. 2   St Bride's church, Fleet Street, EC4: recording the walls of the late thirteenth to early fourteenth century crypt. (R. Bartkowiak: London Archaeological Research Facility)

Shepherd. Before work began on the final report of the St Bride's site, it was noticed that, although a sequence of plans survived from the original excavations, there was no trace of any detailed elevations of the medieval walls exposed during that work. Since it was thought that such stone-by-stone records of the walls might materially assist in the resolution of the dating of the medieval phasing of the church, it was decided that the elevations should be drawn. Permission to undertake this recording work was sought from the present rector, Canon John Oates, and was willingly given. The tolerance with which he and his staff greeted our dusty endeavours is gratefully acknowledged. The undergraduate team from University College London which was brought together to undertake that work was directed by the authors. Our records will ultimately be presented to the Museum of London, under the site code SBC92.

Since one of the problems with the RMLEC field records is the lack of absolute levels related to Ordnance Datum, the first task of the U.C.L. team in December 1992 was to establish a series of bench marks in the crypt, providing a common datum for all the medieval walls exposed. After this, walls were cleaned (Pl. 1) and stone-by-stone elevations were drawn at 1:20 noting features such as coursing, stone type, tooling and mortar (Pl. 2).

Work initially concentrated on the 16 m length of the external face of the east end (Pl. 3), which still had traces of its medieval plaster rendering into which lines representing coursed ashlar had been incised. Then attention turned to the internal elevations of the north aisle wall and the late thirteenth to early fourteenth century crypt which survived to full height, complete with a blocked door, part of an original window and a chalk block vault supported by two Reigate stone ribs (Pl. 4).

Records were also made of the fragmentary remains of the free-standing twelfth century bell tower, which now lie in the vaults beneath the south aisle and not on public display. An elevation was then drawn of the south face of the base of the seventeenth century tower, which was found to lie over a medieval foundation still just visible in the crypt today but unrecognised prior to the recent study. The final part of the first season saw preliminary work begin on a catalogue of the architectural fragments derived from the medieval church which were recovered during the 1952–4 excavations, and are still stored in the crypt. Moulding profiles were taken which, when compared with similar features recorded elsewhere, may help to date more precisely the major construction phases of the church (Pl. 5). In addition to the moulded stones, fragments of two Purbeck marble grave covers and a Bath stone font were also recorded. It is to be hoped that a further season of work in the church will be mounted, to complete the first phase of the catalogue of architectural fragments (Pls 6 & 7).

THE DEVELOPMENT OF ST BRIDE'S CHURCH

The provisional results of the first season have proved most interesting, for it is now possible to suggest a rather different chronology for the development of the church than that suggested in 1960 in the crypt display, in Professor Grimes' summary report published in 1968, or in the guides currently on sale (Morgan, 1987: 7). The full details of the proposed sequence will be discussed and illustrated in a major report being complied by the Museum of London for English Heritage. In that volume, a re-interpretation of the Roman, sub-Roman and early

Pl. 3   St Bride's church, Fleet Street EC4. External face of the east end of the church, showing twelfth-century buttress with medieval external plaster-work still *in situ*. The 5 x 100 mm scale rests on the off-set foundation. (R. Bartkowiak: London Archaeological Research Facility)
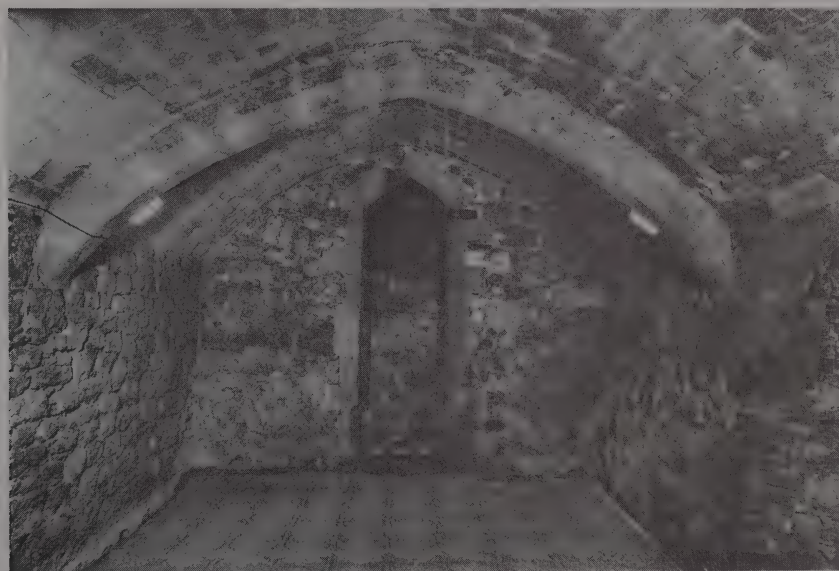
Pl. 4   St Bride's church, Fleet Street, EC4: part of the photographic record made of the walls of the late late thirteenth to early fourteenth century crypt. (R. Bartkowiak: London Archaeological Research Facility)

medieval levels will be offered together with a re-assessment of the development of the masonry church. However, a summary of some of the main points in the later medieval sequence is provided below:

a) the ragstone rubble, flint and tile foundation once thought to represent the western porch of the first church (cf. Grimes, 1968: 185, Fig. 42) is now seen as part of a separate building, possibly mid-Saxon, but certainly earlier than the late Saxon masonry church;

b) the free-standing mid-twelfth century tower was probably built to function initially with the apsidal-ended church (cf. Grimes, 1968: 188–9, Fig. 44), rather than the square-ended chancel;

c) the chapel to the south of the chancel was probably built *before* the chapel on the northern side;

d) the surviving vaulted crypt in the north-east corner of the church was inserted below the northern chapel as a later development (cf. Grimes, 1968: 190–91, Fig. 45);

e) the twelfth century tower may have been dismantled in the fifteenth century, when its sub-surface remains were incorporated into a vault or crypt;

f) a replacement tower seems to have been erected at the west end of the church, presumably in the fifteenth century, but certainly before the Great Fire (cf. Grimes, 1968: 189–90). It could now be argued that the Wren design for the post-Fire church mirrors the fifteenth century plan rather more closely than previously thought, since even the positioning of the tower with its world-famous steeple was dictated by the earlier work, if our identification is correct.

CITY CHURCHES BUILDING RECORD

It is now hoped that U.C.L. teams will be able to mount similar exercises on other churches in the City, as part of a long-term project to record the remains of medieval masonry which somehow survived the consequences of Dissolution, the Great Fire, the Blitz and major restoration programmes. These monuments include St Bartholomew-the-Great, All-Hallow-by-the Tower and St Mary-le-Bow in Cheapside. This project, the City Churches Building Record, is supported by the London Archaeological Research Facility, an independent agency which promotes co-operative archaeological ventures between the Museum of London and University College in particular. An advisory board was set up under the chairmanship of Professor James Graham-Campbell in November 1992 to monitor the work and to raise funds for its activities.

The first project in this programme was a small-scale recording exercise at St Vedast, which lies at the junction of Foster Lane and Cheapside (Pl. 8). This church, like St Bride's, had been gutted in the Great Fire of 1666 and in the Blitz in 1940. Stone cleaning on the south face of this church in 1992 had exposed a substantial section of pre-Great Fire walling which had been incorporated in the later reconstructions. According to the designs for the late seventeenth century church, that wall was not intended to be exposed, since the Portland stone cladding does not extend over the entire south wall. As a consequence, fragments of the earlier church which were rudely absorbed without embellishment in the later repair work are

Pl. 5 St Bride's church, Fleet Street EC4: Sarah Semple (U.C.L.) recording mouldings on architectural fragments recovered from the 1952–4 excavations. (R. Bartkowiak: London Archaeological Research Facility)

Pl. 6 St Bride's church, Fleet Street EC4: architectural fragment recovered from the 1952–4 excavations, perhaps from a screen. Scale: 2 x 100 mm. (R. Drinkall: London Archaeological Research Facility)

now on public view. Archaeologists from the Institute of Archaeology, U.C.L., drew a detailed elevation of the south wall with its blocked doorway while the scaffolding was still in place in November 1992, and completed their records in February 1993. The project was directed by the authors for University College, and the records will be presented to the Museum of London archive, under the site code: SVC92.

The discovery at St Vedast demonstrates graphically how Wren's churches incorporate older features, and add to a growing list of late seventeenth century City churches which are not all they seem. Although 87 churches were burnt out in the Great Fire, only 51 were rebuilt afterwards (Milne, 1986: 120–24). Of those, some were thought to be not so badly damaged as to require complete rebuilding. In addition to St Vedast, the churches of St Margaret Pattens, Allhallows the Great and St Christopher-le-Stocks are known from documentary sources to have undergone only relatively minor repairs before being pressed into service again (Reddaway, 1940: 124). The fabric from before the Great Fire was therefore initially preserved in all of these examples, although St Christopher's was subsequently demolished in 1782, and Allhallows in the late nineteenth century. Archaeological research has shown that other late seventeenth century churches such as Christchurch Greyfriars (Herbert, 1979), St Swithuns (Grimes, 1968: 199–203) and St Mary-le-Bow in Cheapside were built directly over the remains of the earlier foundations, and thus accurately encapsulate part of the medieval townscape, while some such as St Mary at Hill (Lea, 1986: 22–4), St Martin's Ludgate (Richardson, 1983: 275), St Albans Wood Street (Grimes, 1968: 203–9) and St Vedast also made use of upstanding masonry from the previous buildings. Such was the shortage of building stone in London after the Great Fire that all reusable materials from churches (and secular structures) which were not themselves rebuilt were readily recycled in those which were.

The extent to which London's medieval ecclesiastical fabric survived the Great Fire has yet to be quantified. An indication could be sought in the accounts for the rebuilding of London's churches (Weaver, 1915), which show that costs ranged from a mere £1,853 for St Vedast to over £11,000 for such buildings as St Bride's, Christchurch and St Lawrence Jewry. It seems reasonable to assume that the cheaper programmes, that is the 37 buildings which cost less than £6,000, would seem more likely to have incorporated upstanding fabric than the more expensive: indeed, the costs for St Vedast (£1,853), St Christopher (£2,098), St Alban (£3,165) St Margaret Pattens (£4,986) and Allhallows the Great (£5,641) bear this out. It could therefore be argued that, leaving aside the brick buildings, any masonry church which falls within this lower price range might also be a candidate for substantial medieval survival. Richard Lea's recent investigations at St Mary at Hill (£3,980) lends welcome support to this suggestion (1985).

The full significance of Professor Grimes' work as St Bride's then becomes clear, for that was probably the most expensive church of all, if the cost of the tower and spire is included. Even working with such a relatively lavish budget, the design and proportions have still consciously incorporated much of the late medieval design, rather than branching out into radically fresh fields. Thus if even the most expensive post-Great Fire redevelopments accurately incorporate, at the very least, the plan of a medieval church, it can be confidently

Pl. 7   St Bride's church, Fleet Street EC4: medieval Purbeck marble grave cover recovered from the 1952–4 excavations. (R. Drinkall: London Archaeological Research Facility)

Pl. 8   The City Churches Building Record: Nathalie Cohen from U.C.L. drawing the recently-exposed blocked doorway in St Vedast church, Foster Lane, EC2 in January 1993. (R. Bartkowiak: London Archaeological Research Facility)

claimed that further fragments of London's ancient buildings survived the Fire and the Blitz, but remain masked within the core and foundations of Wren's churches. They await discovery, perhaps when walls are cleaned, replastered or repaired, when neighbouring buildings are demolished, or when floors are relaid.

A similar picture has emerged in other London churches which were not themselves touched by the Great Fire; a prime example is the recent work at Westminster Abbey. Here, careful recording by Tim Tatton-Brown has shown that the lower *c.* 15 m of the two west towers rebuilt to a design by Nicholas Hawksmoor in the eighteenth century, still incorporate substantial sections of the twelfth century church which had previously been thought to have been rebuilt in the late medieval period (Tatton-Brown forthcoming). The search for London's past therefore continues above as well as below the ground.

Abstract

During the winter of 1992–3, a team from the Institute of Archaeology at University College London recorded medieval features which survived beneath the St Bride's church in Fleet Street, London. The project was a collaborative venture with the Museum of London designed to provide additional information for a report currently being prepared on the late Professor Grimes' 1952–4 excavation programme, as well as for the new exhibition in the crypt. It also formed part of a larger programme, the City Churches Building Record, which aims to prepare detailed records of all the surviving (if fragmentary) remains of medieval churches in the City of London, such as that recently exposed at St Vedast, Foster Lane.

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BOOK REVIEWS

AMBROSE Timothy and **RUNYARD** Sue (eds). *Forward Planning: A handbook of business, corporate and development planning for museums and galleries.* London: Routledge, 1991. 169 pp. ISBN 0-415-06482 (hbk), 0-415-07026-0 (pbk). £22.50.

Ten years ago forward planning in museums consisted largely of timetabling the following year's exhibition programme and ensuring that money was available for additional projects to be undertaken in that period. Even though government-inspired changes were beginning to occur in the public sector, at that time it would have seemed implausible to the average museum curator that a book totally dedicated to the issue of forward planning in museums could be produced. With the subtitle 'A handbook of business, corporate and development planning for museums and galleries' the likely assumption would have been that the book was intended for the independent museum sector that had blossomed during the 1970s and which, through the need for financial solvency, would have to take such non-museum type issues on board. However, by 1991 the environment in which public sector museums operated in the UK had changed radically and when this book was published jointly by the Museums and Galleries Commission and Routledge it was welcomed by both public and private sectors.

As a result of a series of govermnent acts passed in the 1980s all publicly funded services have been encouraged to show that they provide 'Value For Money'. The recent Audit Commission report *The Road to Wigan Pier*, London, HMSO (1991), which concentrated specifically on local authority funded museums, has added to this pressure by making senior local government officers aware, often for the first time, of the fact that museums can be managed, monitored and controlled in much the same way as any other public service. One result of this report and subsequent Audit Commission audits of individual museum services, has been a need for museums to plan all areas of performance, from cataloguing collections to building maintenance, in such a way that it can be measured by bodies outside the museums management—and that will ultimately provide the proof of 'Value for Money' to the public.

The publication of *Forward Planning* was thus considered timely by many who were having to grapple for the first time with basic elements of management, such as planning and monitoring, in a formal fashion. For many curators awaiting their museum audit, it was hoped that the book would provide a step-by-step guide to forward planning that could be followed as a formula within each museum (similar to the approach put forward by Amhurst H. Wilder Foundation in the area of marketing planning in *Marketing Workbook for Non-Profit Organisations*). The book does not, however, fulfil this 'coal-face' function, although it does have sections which summarise the planning stages such as the contribution by Patrick

Greene and Robert Scott and the final, anonymous, 'Checklist of tasks'. Instead it operates at one remove—aiming to convince the reader of the need for forward planning and highlighting related issues such as its use in monitoring and evaluation of performance. Whilst sympathising with those who wanted a simple, Museums and Galleries Commission approved, step-by-step approach which they could follow mechanically, the editors are to be congratulated for presenting a simple but broad discussion at this initial stage of the profession's understanding of the subject. The book, with its short 'further reading lists', provides a basic introduction to the practicalities of forward planning in the museum context for students on museum studies courses and curators encountering the subject for the first time. However, as people working in the museum field have to proceed rapidly from understanding wider issues to using planning in everyday management, a formulae planning guide, perhaps aimed at smaller museums, would be a useful next publication from the MGC especially as Area Museum Councils and the MGC intend to use museums' forward plans in future grant assessment procedures. Conversely, those wanting a greater theoretical understanding of planning as a management tool would be advised to consult one of the many general management text books that are available. Alternatively they should await the forthcoming book by Dr M. Fopp (Director of the RAF Hendon Museum) on museum management.

Forward Planning is divided into four main sections which lead logically through the planning stages. From the initial assessment of a museum's current position the reader is guided through the process of deciding how to write the plan and the specialist areas to be considered in forward planning for museums, to a final, limited, section on uses to which plans can be put in terms of the assessment and monitoring of peformance and day-to-day management. Each of these sections is made up of short, two to six-page contributions by 25 writers drawn from within the museum profession and from leisure, tourism and heritage consultants. Two additional sections containing 'samples and extracts from forward plans' and 'checklist of tasks' function primarily as appendices and summaries to the main sections. The format is a little clumsy as it requires the reader to make the connection between the different contributions in each section. A useful addition to the book would have been an introduction or summary to each section that could draw together common threads and indicate where the contribution fits into the planning process. In particular it would have been interesting to know why Ian Walden's contribution on Performance Indicators was put in the 'Forward planning in specialist areas' and not in the section on 'Using your plan'. The 'Forward planning in specialist areas' section was particularly bitty with too many frustratingly short contributions whilst the 'Using your plan' section was far too brief to be useful. Planning can only be of value to a museum if it is an integrated part of the management cycle. The much sought after efficiency, economy and effectiveness does not result from the planning process alone—it therefore seems unbalanced to have twenty-three contributions relating to the planning process and only two concerned with the use to which the planning is put.

The individual contributions are too numerous to discuss separately—but a few can be highlighted as being useful. The two

longest items in the book, by Ron French, 'Frameworks and formats; getting started', and by Patrick Green and Robert Scott, 'A step-by-step guide' provide practical advice on different types of planning, their use in museums and specifics of the planning process. Victor Middleton and Ken Robinson's contribution 'Working with consultants' should be put on all museum studies reading lists, not just planning ones, as it contains information essential to those wishing to employ consultants in the museum field.

Finally, anyone reading this book cannot fail to be impressed by the full-page portraits of each contributor. Ian Beesley is a first-rate photographer with an outstanding ability to capture an individual's personality. The portaits are so good that it would be easy to imagine that the book was conceived of as a photographic 'Who's Who' of the British museum world—with the associated articles an afterthought. In the section 'Forward planning in specialist areas' where the contributions only average *c.* 1,000 words this idea seems extremely likely!

To conclude, the Museums and Galleries Commission produced a timely book largely in response to changes in the funding and philosophy of the public sector during the 1980s. It was designed to encourage museum professionals to undertake forward planning in a way suitable to museums rather than by borrowing management methods from other fields. It is not a museum management text book—nor is it a comprehensive 'how-to' workbook. The book has a message that was new to some curators at the end of the 1980s/early 1990s—and reinforced this message by bringing in a range of the 'great and the good' via their writing and their portraits. However, less than a year later the limitations of this publication can

be seen. More detailed museum planning books have begun to appear, such as *The Manual of Museum Planning*, 1991, Lord, G. and Lord, B. eds, whilst the need for curators to proceed to use planning in management slightly negates the proselytising role of this book. In fact, ironically, it may be the rather loosely tied selection of contributions in the 'Forward planning in specialist areas' that is of most enduring interest, if only as a reminder of the breadth of areas to be included in museum planning.

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RANDSBORG, K. *The First Millennium* AD *in Europe and the Mediterranean,* Cambridge: Cambridge University Press, 1991. 230 pp.; 83 figs; 9 pl. ISBN 0-521-38401-X. £11.95 (pbk).

RICH, J. and **WALLACE-HADRILL**, A. (eds). *City and country in the Ancient World*, London: Routledge, 1991. 293 pp., figs. ISBN 0-415-01974-5. £11.99 (pbk).

STEPHENS CRAWFORD, J. *The Byzantine shops at Sardis. Archaeological exploration of Sardis* 9, Cambridge, Mass.: Harvard University Press, 1990. 150 pp., 96 pl. ISBN 0-674-08968-5. £39.95.

HODGES, R. *The Anglo-Saxon achievement*, London: Duckworth, 1989. 202 pp., 56 figs and pl. ISBN 0-7156-2130-0. £12.95 (pbk).

The idea of reviewing these four books together was as follows. Randsborg, always strong on material and ideas, would give guidelines by which to study the millennium.

City and country in the Ancient World would cover the whole area under discussion from the last centuries BC to the third century AD. Then, when East and West go their separate ways, Sardis would cover the East, high on facts but low on ideas, and Hodges and the Anglo-Saxons would stand for the West, high on ideas and low on facts. Each book was reviewed briefly after it was read, and the effect was therefore cumulative.

Randsborg lived up to expectations on what he attempted but, inevitably in such a pioneer work, failed to deliver a totally convincing product. He has for years been trying to inject some real archaeology into the study of the Vikings to the dismay of the art historical/historical axis of older Vikingists. Anything that he writes deserves more thoughtful consideration than they have ever given him. This is clearly a warning to Romanists that they must think through his ideas rather than simply dismiss him when they find that his chosen material is partial, or his conclusions fail to agree with their historical prejudices.

His book gives us a historical framework, geographical survey, a look at rural settlement and towns, production and exchange, society culture and mentality, and archaeology and historiography. This means that most of the book is taken up with a series of snapshots of material from which the reader has to draw self-made conclusions. Where there is an attempt at synthesis and summary it does not often stray far from description, and rarely achieves analysis. I get the feeling that this grows from a failure of my own to embrace all the right fundamental texts. If I had read the proper theoretical works I might come to the proper syntheses for myself upon reading this series of summarised material.

I find two main problems with the book, both of which stem from its valiant attempts to do the impossible. The material quoted is only as good as the sources from which it comes, and there is no space in such a short survey to set out the bases of the theories used, or even to set out a theoretical position from which to start. Thus on pp. 57–8 mention is made of the distribution of mosaic pavements in France through time; they belong mostly to the period 100 to 350 and deductions are drawn from this. Yet I would risk a fair amount of money on the guess that not a single one of those mosaics is actually dated archaeologically with a known margin of error. I strongly suspect that they are all classified and dated by waffly art-historical means. This means that at best they have a floating chronology which may be up to two centuries out on an absolute scale. I am not alone in thinking this for there is now a strong movement in southwest France which suggests 'if our pavements seem to be dated to the fourth century, and we know from a variety of sources that villas were thriving in the fifth century, perhaps we should change our dates.' This problem of a floating dating from art-history or archaeology set against absolute historical dates is a likely source of difficulties throughout Randsborg's text.

An example of the second problem crops up commonly with the mention of State Formation. It is clear that this is a Good Thing which explains the rise of the Middle Ages from the Slough of Despond created by the Fall of the Roman Empire, but this is a belief which I am unable to share both through ignorance of the basic bibles, and a clinging anarchy which doubts whether the rise or fall of institutions ever explains anything that matters. My over-enthusiastic use of

capitals is meant to suggest that the capital-
ised entities are myths which have to be
demythologised before they can be used in
any useful and rational discussion. Any reli-
gious overtones in this section are fully in-
tended.

These problems and doubts notwithstand-
ing I moved on to the second book with a lot
of new ideas to experiment with, and some
old ideas strongly reinforced. I did not have
the required method for studying the millen-
nium, but I had some useful buildings blocks.

City and Country comprises eleven pa-
pers which, if one were feeling sadistic, could
be marked from 10 to 0. Snodgrass (Archae-
ology and the study of the Greek city) is the
clear winner for temperance, incisiveness,
humour, and downright uncommon sense.
He also includes a change of published view
on the interpretation of burials which is an
object lesson in moderate, reasoned, re-think-
ing. His concern, shared with Morris (The
early polis as city and state), and Rihll and
Wilson (Modelling settlement structures in
Ancient Greece), is to consider the origins
of the Greek city in the process of
synoikismos, or coming together of non-urban
communities. All three papers, whether
analysis of the archaeology by Snodgrass,
discussion of historical sources and archae-
ology by Morris, or mathematical modelling
by Rihll and Wilson, come alive through an
open-minded interest in what they are doing.
But none of them makes any mention of the
possibility that towns had ever existed be-
fore the Greeks invented them, and the no-
tion that the idea of towns may have drifted
over from the Near East is clearly not to be
mentioned in polite, Greek-based, society.

Cavanagh (Surveys, cities and synoecism),
and Patterson (Settlement, city and élite in
Samnium and Lycia) both left me uncertain

what their message was, or whether it had
application beyond their own chosen limits.
Osborne (Exchange and Society in the Greek
city) told me more than I wanted to know
about the details of several Athenian busi-
nessmen, and Corbier (City, territory and
taxation) told me a lot about taxation, but
less about its effect on cities. Wallace-Hadrill
(Elites and trade in the Roman town) gives a
useful analysis of parts of Pompeii and
Herculaneum in his attempt to remove the
idea that the upper classes were divorced
from trade, and Potter (Towns and territo-
ries in South Etruria) moves smoothly from
the Italian Bronze Age to the Papal States in
a short study of the relationship between
Rome and its hinterland. Millett (Roman
towns and their territories: an archaeologi-
cal perspective) clearly had a difficult job to
explain to an audience heavily based in texts
what field survey really is, and what the
problems and opportunities of the method
are: but, even when watered down for classi-
cists and historians his section makes useful
reading. Perring (Spatial organisation and
social change in Roman towns) neatly and
briefly details a series of thoughts on the
relationship between the physical setting, the
economics, and the changes in social order.

The best of these papers left me with wide
ideas to apply to the millennium; those I
found least useful seemed irremediably bur-
ied in localised textual preconceptions. The
book was described in the introduction as a
dialogue between historians and archaeolo-
gists; unfortunately classical monologues
were still in evidence.

'In the Byzantine shops at Sardis one can
see both an end and a beginning. The world
of classical antiquity had succumbed beyond
all efforts to revive it; the Byzantine Em-
pire's struggle to survive had only begun.'

(p. 125) For this excellent text I can almost forgive the shortcomings of the archaeology at Sardis, but before mentioning the positive side the debits must be mentioned. The writers who compiled this report clearly had an impossible task, and it is to their credit that they show from time to time that they are aware of it, without making a meal of it. The site was dug in the 1950s and 60s in a way that would have been reprehensible 30 years earlier. Stratigraphy was paid only lip service and layers and contexts were not recorded. There is only one section in the report, and that is a 'schematic stratigraphic section', (Fig. 34) which, taken cm by cm probably causes more problems than it solves. Some pottery was kept, but the writers realise, and point out, that because the rate of discard is not known, none of the surviving pottery can be used in any numerical way.

The main problem comes in the interpretation of the row of shops alongside the Bath Gymnasium complex. The deposits in each shop show two falls of roof-tiles with a layer of deposit containing many and varied finds on the floor of the shops and another between the two layers of tile. These 4 layers are 2 m in depth, and each layer is therefore about 50 cm. If, as the writers suggest, the upper wooden floor fell burning in an earthquake of the early seventh century, days or hours after the shop had been evacuated, why are the burnt timbers not resting on a scorched floor? Is the lower deposit in fact the result of slow build-up of squatting and desertion, and are there two fires and earthquakes? We can probably never answer the question from the evidence presented here, but sections were left undug, and they might give the answer.

It would be tempting to look more closely at the large number of coins found, but that needs more time and space than is available at present. The finds are not separated out into lower deposit and upper deposit because that is not how things were recorded; instead, each coin has a relative depth. A first look at these measurements suggests that coins from $c.\,300$ to $c.\,615$ were found throughout the four levels. Some groups of coins which include some of the latest in date are said to have been found directly on the floor. The only detailed mention of animal bones is a problem since there is surely some typographical error: '...identified twenty sheep or goat bones, of which the highest percentages were mandibles and long bones: in other words, lamb chops and shish kebab.'

And yet the writers are aware, especially in the useful chapter by the main author Stephens Crawford on The Byzantine Shops in Context, that Sardis was a point both in space and in time. He compares the shops at Sardis with shops of different dates, both before and after the rather fuzzy date suggested of fifth to seventh century, and with shops at different places from Delphi to Jerusalem. He is aware that they are from a time of change, and he is willing to tangle with the nature and direction of that change. This highly imperfect volume therefore gets marginally higher approval than the City volume which, though high on scholarship, is depressingly fixated on the Graeco-Roman world. And so to the Anglo-Saxons.

Richard Hodges re-interprets Early England in seven chapters. First there is the off-putting 'Archaeology and the origins of capitalism', then the 'Beginnings of English society', 'Inflation, the church and the middle Saxon shuffle', 'The age of Emporia', 'The Carolingian connection', The first English industrial revolution', and 'The genesis of a

miracle'. My expectation of 'high on theory, low on facts' is not really correct, nor is it the main bone of contention. There are many material facts and surveys of facts quoted, but somehow the synthesis starts somewhere on the theoretical side of the facts. The result is a series of interesting ideas which dissolve as soon as they are subjected to detailed scrutiny or wider material testing.

Some of the facts surprise me; that Gatcombe, near Bath, where no 'great house' actually survives in whole or part can be quoted as a major Roman country house; that St Cuthbert took with him to the grave a large amount of disposable valuables; that continuity at the Rivenhall villa in Essex is no longer contentious. A few points escaped me—just when was Hamwih flourishing and in decline; it seems to vary at different points in the book. Some points annoyed me—the use of Germanic styles of ornament in Roman contexts in the late fourth century. And a lot is done with the changes in the coinage without ever trying to trace a comprehensible course from the late Roman system through the Merovingian coins to the Empire of Charlemagne.

This, clearly, is to miss the significance of the book which talks of Industrial Revolutions, and the forging of the English character. But the major problem for the material-based reader and the sceptical interpreter is the connection between the material quoted and the theories propounded. A further problem is the highly divided attitude to written sources. It is probably too cynical to say that Hodges rejects historical sources for the fourth, fifth and sixth centuries, where they disagree with him, and swallows them whole for the seventh to tenth centuries when they help him. He goes so far from his professed archaeological tenets as to name Ine as the

mind behind Hamwih, and Alfred as the progenitor of the Industrial Revolution. I neither agree, nor disagree, I am simply unable to cope with ideas which have strayed so far from the material on which they are said to be based.

That is the easy part, to review the four books. Now, since they clearly do support and contrast with one another in many different ways, strands need drawing out, and a final pattern of future study needs designing.

Boundaries must form one of the first contrasts. The only detailed excavation report might be expected to be the main offender, yet it is commendably aware of its place in space and time. It is some of the surveys of the ancient city which show frightening myopia. The Athenian sources seem to absolve their students from looking outside; sources also free Hodges of any worries that the few published continental early medieval sites may not necessarily be typical. In these source-bound cases the argument is presumably that a case study of what is known will be useful for comparison with what is only archaeology.

Beyond either the site, or the written source, there are problems with the area under study. The general assumption is that study areas must be coherent. Thus it is clearly felt to be pointless to waste time comparing Byzantine shops in the eastern Mediterranean with postholes in eighth-century Scandinavian emporia; compare like with like. In the same way, the city in the ancient world clearly does not include concentrations of mud huts, even if they are enclosed within barbarian copies of Hellenistic walls and towers. Yet this is just what Randsborg to his great credit, and inevitable partial failure, tries to do.

One theme that could be seen running through all four books concerns the climate necessary for the establishment and maintenance of towns and cities. For Randsborg this seems to be basically a matter of strong organisation and peace; Snodgrass and company seem to see it more as a matter of reaching a critical population density from which the need to organise and police grows; Crawford hints that units like shops die out where there is Islamic, or any other successful, invasion, while they carry on in places like Corinth; Hodges does not explain why towns in Roman Britain give way to aboriginal subsistence, nor does he really try to explain why towns appear again in Saxon England. The question which immediately comes to mind is whether the Greeks who see towns appearing for the first time ever (more or less *sic*), have anything to discuss with post-Roman archaeologists in northwest Europe? In other words, is the move towards towns in Greece similar to the move towards towns in early medieval Europe? This immediately leads on to a whole range of questions. Is the development of the city in Greece a home-grown new idea, or could there be outside influence? Is the first formation of cities a process similar to the re-creation of towns? Are towns between 1000 BC and AD 1000 similar enough to deserve comparative study?

This suggests a major stumbling block to future studies. Excavation concerns a site and sometimes results in a site report. If it is a rural site it does not include stratigraphic material on a town, and vice versa, unless the rural site develops into a town on the same spot. Synthesis usually concerns a period with, at best, a nod in the first chapter at what went before, and in the last chapter, to what came after. Some of this comes from the nearly extinct methods of teaching archaeology by periods rather than by themes— a nasty legacy from the past which lingers on in a few distinguished haunts. It surfaces at nearly every conference where people want to talk about what THEY know, rather than what WE, collectively, do not know. It is the rare Randsborg who is not afraid of spreading himself thinly and exposing the gaps in his knowledge, and this rarity points up the value of his attempt.

A second stumbling block concerns interpretation. Crawford at Sardis sticks closely to his shops, compares dimensions and plans, and writes as if Interpretation were totally unproblematic. Hodges interprets both primary and secondary sources (i.e. sources already once interpreted) with abandon, and so flies so far from any test as to invite polite disbelief. Randsborg probably sticks fairly closely to a set of interpretative principles, but I do not know them well enough to be sure, and if you dissent from those principles much of what he says is open to doubt. Snodgrass and company, and the archaeological Romanists, probably steer the easiest course to follow and to check on, for they are aware of the problems of interpretation and try to cope with them as they go along.

This needs to be taken further, for all interpretation of archaeological material has to be done by analogy. Some will try to use the analogy of contemporary written sources still not understanding that this reduces two potentially good and separate sources of evidence to one muddled and poor source. The vital question then becomes the chosen point in time and space with which to compare the other point in time and space which is the material that needs interpreting. Kevin Greene, in his new British Museum publica-

tion, *Roman Pottery* (1992: 61) quotes Hugo Blake, who has constructed important parts of our knowledge of medieval pottery in northern Italy, as writing of the ancient world 'the ceramic structure (in terms of qualities and distribution) is unparalleled until the eighteenth century when the social structure of wealth and aspirations was remarkably similar to that portrayed in the Roman empire.'

On a completely different subject, John Smith, expert on English building plans and roofs, has caused trouble in Roman archaeology by interpreting the plans of Roman villas on the same principles which guide him on medieval houses. Or is it Elizabethan mansions? Or should it be, to blend with Hugo Blake, the eighteenth century stately home? I have not yet succeeded in making John Smith tie down his interpretations of Roman villas to one comparative spot in time, but since whatever time he chose, he would be deluged with criticism just for that choice, he may well be wise to avoid it. But, just like the use of pottery evidence for constructing trade patterns and understanding the economy, it does matter. Pottery evidence means something quite different if interpreted in the light of sixteenth century parallels as opposed to a fourteenth century model. In exactly the same way a plan of part of a medieval hall-house means something quite different about social structure from a similar plan which is part of an eighteenth century stately home.

This raises the spectre of uniformitarian assumptions about which people really must be more explicit. If the material remains of Roman pottery really are comparable with the eighteenth century is it legitimate to assume a total economy in the ancient world similar to that of eighteenth century Europe?

If that jump cannot, or should not for good reason, be made then economies cannot be reconstructed from pottery, and so all attempts to talk about the Roman economy from pottery should cease. The same with house plans; in fact the same for interpretation in general.

To get back to subjects covered by our four books, the subject of population density brings out this problem in very severe form. The assumption lurks throughout the four books under review that social complexity is linked directly to population density. Where that density is low towns will (can?) not appear; where density is rising, society will need to become more complex to cope with the strains produced by people living at close quarters; where density is high, towns are inevitable. This may make sense to the town lover. What worries me, as a town hater, is that the converse is often taken to be demonstrated. When towns decline this clearly shows a drop in the numbers of people in a given area, whether in Asia Minor, or Britain. Since we know (full alert—this word always signals trouble) that there was a population fall in the later Roman Empire, the decline of towns is clearly to be expected. And since the towns do in fact decline, this clearly demonstrates the fall in population.

To pass responsibility for explaining social and economic changes on to population studies is, for the archaeologist, very helpful, because the population specialists admit that we simply do not know what makes populations increase and decrease in significant terms. Wars and invasions are poor explanations since there are so many examples in well-documented periods in which wars have caused an instant fall in numbers, quickly followed by a healthy population rise.

What then are the points for the future? At its simplest, just what was going on, and how can we understand it? This modest programme of research concerns western Europe and the Mediterranean area from 1000 BC to AD 1000 and aims to provide models of change which can be progressively tested, either to destruction, or to improvement. Why, for a start, are towns of the earlier first millennium AD in north-west Europe confined to the Roman Empire? What is the comparable picture in the south-east corner of the Empire? To what extent are the Barbarians different from the Romans? Is it a matter of social structure, or gracious living, of living in differently shaped boxes, or living differently shaped lives? Since seeing the indigenous material of the Roman period in Poland, and the Roman imports, I want to know whether the barbarians were happy in their way of life provided the Empire was there to pass on some of the better material things, like silk and pepper. Are there two different peoples with two very different aims and lives, or are the Romans simply those with an overstructured life who produce, and the barbarians those who are footloose and consume? Should we understand the Empire and the Barbarians as materially rich but spiritually deprived townies versus the feckless Travellers?

This type of model, in which the trappings of Empire are not only reduced in importance, but positively scorned, would have a major contribution to make in the Dark Ages. It would in fact define Dark Ages as those periods of time in which human beings were free of the drag of history and the need to keep up with other so-called civilisations. The questions would then reverse themselves, for we would want to know not why people failed to stay civilised, but why they were ever perverted into brief bursts of civilisation. For civilised read civicised throughout.

If my 'back to nature' plot does not carry conviction I must point out that it sets firmly on the sceptic the need to fight it with a firm clear model of why people apparently so easily gave up a surplus of food, warm waterproof houses, and all the other concomitants of civilisation, which I am trying hard to list without success. Money and a market economy? A police state? Taxes, inevitably the greater the larger the unit to which you belong? There are of course other benefits which sometimes attend civilisation, but they do not necessarily avoid simplicity; they include peace, prosperity, art, religion, basic simple medicine (at this date mainly herbal), and the exchange of goods.

This of course leads on to the ultimate question of why the barbarians moved into the Empire. There are two reasons why this area of study is such a mess. One concerns modern academic politics, the other concerns modern national politics. Neither has any bearing on the sources available or the possibility of finding interesting and testable answers. Academic politics insist that periods have clear boundaries and academics must be specialists. The number of people who can write with authority both about Italy and the Empire, and the northern Barbaricum, in the fourth century AD is very small indeed. Most of them, I suspect, are in what used to be eastern, and is now central Europe. Modern national politics has kept these people away from our conferences, has kept much of their writing untranslated in Slav languages, and so has kept us in ignorance of much of the recently gathered evidence. This should be an area where exchange of ideas and visits will radically change our knowledge in the next decade.

Basically there seem to be three models, all of which need hard scrutiny from all sides, Roman and Barbarian, East and West. In the first model the Empire was a highly desirable property which primitive barbarians could not even hope to purchase. Gradually the property ran down, the barbarians were still interested, and by the late fourth century the balance of power was so even that a barbarian push caused the property to change hands. The second model would start with a roughly even balance of power at the time of Augustus, two spheres of influence which neither side had the extra power to conquer. By the fourth century the balance of power had swung towards the barbarians who simply took over an ailing business, put in their own top managers, and ran it their own way. Coming rather a weak third is the balance of power disturbed in the fourth century by terrible hordes of Huns from the East. This sounds rather like a threatened Oriental take-over which forced amalgamation between previously antagonistic western businesses.

All three models are sadly lacking in correlation with published evidence. They are also sadly simplistic in that they take the Empire and the Barbaricum to be stable units when this is clearly not the case. A survey of what was going on in north-west Europe between AD 300 and 450 would show a series of changes in groupings of material culture both inside the old imperial limits and without. In Poland, for instance, either fashions are on the move, or the people who followed certain fashions are moving about. Some cultural groupings, types of finds, patterns of burial rite, leave the areas where they have been fixed for centuries, and move south towards the Empire leaving areas with no visible settlements (Wielbark and Przeworsk). On the other hand some cul-

tures stay put (West Balts). Some useful references on recent Polish work will be found in the recent Polish issue of *Antiquity* (Vol 65, no. 248, September 1991). Then again, the Empire of the fourth century is a different set of cultural groupings from that of the first century. The area-wide changes of the fourth and fifth centuries which are so obvious need to be set in line with the changes of the first to fourth centuries which are far less well studied.

Prime movers in these sets of changes will no doubt be discovered and claimed. Randsborg rather favours the climatic changes which are known to happen during the first millennium. Population changes have already been mentioned, but they can hardly have the status of prime movers, even if they are clearly going to be very important when they do happen. Randsborg sets them second to the climatic changes, a means of passing the effects of climate on to social and political structures. A prime mover which appeared too late to be taken into account in any of the reviewed books is that which Chris Going has outlined in a recent issue of the *Oxford Journal of Archaeology* (Vol 11, no. 1, March 1992). On this account, provoked by the irregular occurrence through time of Roman pottery in British contexts, all economies are unknowingly borne along on waves of boom and bust. Some of the waves are of minor amplitude and short duration, others of greater moment and duration. The fourth century AD was so disastrous because a minor blip on the economic landscape was super-imposed on a long-term decline. And both were inherent in the whole nature of economies and therefore inevitable. So, in the end, was it a major recession which brought my non-Roman Travellers and my Roman Townies to an equal footing?

The point at issue is not whether my subversive model of simplicity, or the standard model of civilisation, is correct, but what the alternative models are, how they work, how well they interpret the archaeological observations, and how they are to be tested against the material. The four books under review form a very useful quartet because, when viewed together they pose all sorts of questions which historians are totally incompetent to deal with, since they are blinded by their sources, and which archaeologists have scarcely begun to consider.

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Notes to Contributors

Papers on any aspect of archaeology may be considered for publication in the *Bulletin*. All contributions and related correspondence should be addressed to the Editor, Professor J. J. Wilkes, Institute of Archaeology, 31–34 Gordon Square, London WC1H 0PY. The following notes are provided as a guide to intending contributors in the preparation of their material.

1) *Typescripts* should not normally exceed 10,000 words in length. They should be typed on one side only of A4 size paper (approx. 30 cm by 21 cm), using double spacing and leaving wide margins (at least 4 cm on the left). Two copies of the typescript should be submitted, the author retaining a third copy (complete with duplicate figures, plates and tables). If possible, authors are asked to provide, in addition, a copy of the text on disk (preferably in Microsoft Word for Macintosh or ASCII files).

2) *Footnotes* should be avoided as far as possible. If any are judged to be absolutely necessary, they should be typed on a separate sheet, not at the foot of the page to which they refer.

3) *Bibliographical references* should follow the so-called Harvard system. The author's last name, date of publication and the significant page number should be given in brackets in the body of the text, e.g. (Sharma, 1973: 29), or, if the author's name has been cited, simply (1973: 129). Full references should be listed alphabetically according to the authors' names at the end of the paper. For example:

for a **paper**:

Sharma, G.R.   1973   Mesolithic lake cultures in the Ganga valley, India. *P.P.S.*, 39: 129–146.

for a **book**:

Butzer, K.W.   1972   *Environment and Archaeology*. London: Methuen.

for an **article in a book**:

Bordes, F. 1973 On the chronology and contemporaneity of different palaeolithic cultures in France. *The Explanation of Cultural Change: Models in Prehistory* (ed. Colin Renfrew). London: Duckworth.

Where the publication cited is a paper in a periodical the title of the journal should be italicized (or underlined) and abbreviated using, if possible, the preferred abbreviations given in the *List of Abbreviations of the Archaeological Literature* published in *Acta Praehistorica et Archaeologica*, 9/10, 1978/9, 271–383. If this is not accessible, there are various other lists which may be consulted, e.g. the ones printed from time to time in the *American Journal of Archaeology*, the *Index des Périodiques* supplement to *l'Année Philologique*, Vol 51, 1982 (for journals dealing with Classical and Mediterranean archaeology chiefly) and the Council for British Archaeology's lists in *British Archaeological Abstracts* (for British periodicals).

4) *Line drawings, plans and maps* should be drawn in waterproof ink on smooth white paper, card or good quality tracing film. Lettering should be neatly done, either by stencilling or using self-adhesive lettering such as Letraset or Presletta, which should be varnished with the spray recommended by the makers to prevent damage. All line illustrations should be at least twice the intended final size, and of a shape suitable for reduction to a full page (17 cm x 12 cm) or half pages (8 cm x 12 cm) size. Line illustrations should be referred to as *figures* and numbered in Arabic numerals. The number should be written on the originals in soft pencil, and should correspond to bracketed references in the text, e.g. (Fig. 3).

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8) *Radiocarbon dates* should be cited uncalibrated, using the convention bp, bc, ad to show that this is so. If calibrated dates are also presented, the convention BP, BC, AD should be used. Standard deviation and laboratory code should always be given.

9) *Abstracts:* a brief *résumé* of about 100–150 words should be supplied with each contribution, and will be printed at the end of the text.

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